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RECENT LITERATURE

BANDING AND LONGEVITY

(See also 14, 69, 87)

1. Migration in Asia as suggested by reported ring recoveries. H. Elliott McClure. 1970. Pp. 85-114 in *Migratory Animal Pathological Survey: Annual Progress Report* 1969. San Francisco: U. S. Army Research & Development Group, Far East.—The first part of this paper summarizes previous literature on migration in eastern Asia, most of which consisted of plotting of hypothetical "routes" of migration. Presumably the arrows on these maps should not be construed as the only flight paths of birds: if so they would literally darken the sky along strategic lines such as the coast of China and the Ryukyu chain. The main part of the paper summarizes 2,623 recoveries of nearly 200 species, obtained from 945,782 birds banded under the auspices of MAPS since 1963. There are many interesting features of these recoveries and of the maps based on them, but the peculiar concentrations of the recoveries—especially swallows in Korea and herons in the Philippines—and the complete lack of reports from China indicate that the geographical distribution of recoveries is extraordinarily biased.

Banding a million birds in seven years, in a program which started literally from scratch, is a great achievement. However, the sponsorship of the program by the U. S. Army will cause some raised eyebrows in these anti-militarist days. Let us hope that some germ-happy general does not take the arrows on these maps too literally.—I. C. T. Nisbet.

2. Operation Baltic. Working Methods. (Akcja Baltycka. Metody pracy.) P. Busse and W. Kania. 1970. *Acta ornithologica* (Warsaw), **12**(7): 231-267. 22 figs., 5 tables. (In Polish, with Russian and English summaries.)—Four pages of very readable summation and information of the work of this major banding project make this paper an invaluable consultant for planners of other large scale projects.—Leon Kelso.

3. Longevity records and banding data on Short-tailed Shearwaters. D. L. Serventy. 1970. *Australian Bird Bander*, **8** (3): 61-62.—Two male and three female *Puffinus tenuirostris*, breeding birds banded in 1947 on Fisher Island, Bass Straits, Tasmania, were recaptured as breeding birds during the 1969-1970 season, 22 years after banding. These five birds comprised 7% of the breeding birds banded in 1947. Since breeding birds are at least 5 or 6 years old, the recaptured birds must have been nearly 30 years old and very likely older.

The oldest birds of exactly known age are three 20 year old birds: a male and a female hatched on Fisher Island in January 1950 and a nestling banded in the same month on Flinders Island. The female first bred at 6 years of age and the male at seven.—Roger B. Clapp.

4. **Weight loss of bands used on Grey Teal.** F. I. Norman. 1970. *Australian Bird Bander,* **8** (2): 29-31.—Analyses weight-loss in a sample of 134 "Sale" bands used on *Anas gibberifrons* in Australia. These bands were "... made of anti-corrosive aluminium alloy (BA.21) in a flat strip of 29 mm by 10 mm (and) 1.35 mm thick" and had a pre-use mean weight of 0.881 (± .009) gm. Weight-losses through the first 4 years averaged 4.8% (0.045 gm.) per annum but these figures do not include two bands, one with a 53.9% weight loss in 6 months and another with a 51.3% loss in 17 months. Great variability in band wear clearly shows that use of these bands on Grey Teal is hardly indicated.—Roger B. Clapp.

5. **New light on the Mourning Dove.** H. Brackbill. 1970. *Maryland,* spring issue: 8-11.—The magazine is sort of the *Arizona Highways* of Lord Baltimore's old stamping grounds. Hervey Brackbill recounts here some of his data collected over 8 years of banding *Zenaidura macroura,* data apparently not published elsewhere. Figures show seasonal fluctuations in numbers, migratory dates, sex ratios, length of stay, etc. In Brackbill's experience, Mourning Doves do mate for life, and at least one is alive 8 years after banding. Since the article ends with a quote in Latin, presumably the editors haven't discriminatory tendencies toward that language; put differently, I still wonder why popular articles never give the Latin name of a species. Someone told me that the Mourning Dove is the only native bird that breeds in all states. In many places it breeds every month of the year. Yet, as Brackbill says "... many details of the dove's way of life are relatively unknown."—Jack P. Hailman.

MIGRATION, ORIENTATION AND HOMING

(See also 1, 2, 32, 37, 50, 51, 52, 53, 54, 55, 69)

6. **Celestial Rotation: its importance in the development of migratory orientation.** Stephen T. Emlen. 1970. *Science,* **170** (3963): 1198-1201.—This important paper is best summarized in Emlen's own abstract. "Three groups of indigo buntings were hand-raised in various conditions of visual isolation from celestial cues. When they had been prevented from viewing the night sky prior to the autumn migration season, birds tested under planetarium skies were unable to select the normal migration direction. By contrast, when they had been exposed as juveniles to a normal, rotating, planetarium sky, individuals displayed typical southerly directional preferences. The third group was exposed to an incorrect planetarium sky in which the stars rotated about a fictitious axis. When tested during the autumn, these birds took up the 'correct' migration direction relative to the new axis of rotation. These results fail to support the hypothesis of a 'genetic star map'. They suggest, instead, a maturation process in which stellar cues come to be associated with a directional reference system provided by the axis of celestial rotation." One important question is left unanswered, however. How would birds orient if they had been exposed to a correct but non-rotating artificial sky? (*cf,* no. **7.**)—I. C. T. Nisbet.

7. **The orientation ability of birds under natural and artificial star patterns. Training experiments with Mallards.** (Über das Orientierungsvermögen von Vögeln unter natürlichen und künstlichen Sternenmustern. Dressurversuche mit Stockenten.) Hans Georg Wallraff. 1969. *Verh. Dtsch. Zool. Ges. Innsbruck,* **1968:** 348-357. (German with English summary.)—Mallards were successfully trained to orient in a planetarium, either with a representation of the natural star pattern or with an artificial star pattern never occurring in nature. Birds trained under part of the natural star pattern were unable to orient when presented with a different part of it. These results suggest that a considerable ability to learn complex visual patterns is involved in orientation, but do not support the idea of a "genetic star map" (*cf,* no. **6**).—I. C. T. Nisbet.

8. **The influence of magnetic information on the orientation of the Indigo Bunting, *Passerina cyanea.*** Stephen T. Emlen. 1970. *Anim. Behav.,* **18** (2): 215-224.—Indigo Buntings did not show any orientation in the absence of visual clues, even in a normal geomagnetic field. Attempts to condition them to

respond to directional changes in an artificially induced magnetic field were also unsuccessful. Nevertheless, Emlen does not regard the question as settled— I. C. T. Nisbet.

9. Spring migratory restlessness in caged birds: a circadian rhythm. J. P. McMillan, S. A. Gauthreaux, Jr., and C. W. Helms. 1970. *BioScience*, **20** (23): 1259-1260.—In spring caged White-throated Sparrows *Zonotrichia albicollis* exhibit a characteristic pattern of intense nocturnal activity and sparse daytime activity. Birds held in constant dim light without external time cues exhibited the same pattern, but not with exactly a 24-hour period, so that the "nocturnal" activity became progressively later on each "night". In the jargon of the trade, this "free-running" of the behavior pattern indicates that it is controlled by a circadian rhythm. The authors discuss how a normally day-active bird can become night-active for migration, and come out in support of Gwinner's concept of two internal oscillators.

At the end of the note they discuss the many published claims that external environmental factors also play a role in the nightly initiation of migration in free-living birds, and state "the evidence we present leads us to reject, or at least qualify, such hypotheses. Our results clearly show that no direct environmental stimuli are required for the nightly expression of activity in spring migrants under constant conditions". This is true enough, but the point is irrelevant to the more complicated behavior of free-living birds. All that their results clearly show is that activity takes place every night in caged birds under constant conditions. They do not show that a bird will migrate every night in the wild, even if its energy reserves permit it to do so. There is at least one important difference between *Zugunruhe* and migration, and the authors' extraordinary reference to "migration in . . . captive birds" suggests that they do not appreciate it.—I. C. T. Nisbet.

10. Displacement and phaseshift experiments with night-migrating passerines. Jorgen Rabøl. 1970. *Ornis Scand.*, **1** (1): 27-43.—Groups of birds caught on autumn migration in Denmark were separated and tested on subsequent nights in Emlen-cages at two different places. Differences in the mean directions taken up by the birds in the two groups suggest that the displaced birds were correcting for their displacement by heading towards a "goal-area", the next staging-point along the migratory route (see *Bird-Banding*, **41** (1): 45, review no **15**). If this interpretation is accepted, the results would suggest that the goal-areas are very close, only a few hundred km ahead of the point of capture. This leads to the concept of the migratory route as an intricate series of staging-points. However, the differences are not clear-cut in all species, and the samples were rather small. An important limitation is that the birds were caught at remote coastal stations: the orientation of the undisplaced birds is intelligible only if it is assumed that they had already been drifted off-course before they were caught. This undermines confidence in the generality of the results.—I. C. T. Nisbet.

11. Flight directions of displaced homing pigeons dependent on the home site and the release site. (Über die Flugrichtungen verfrachteter Brieftauben in Abhängigkeit vom Heimatort und vom Ort der Freilassung.) Hans G. Wallraff. 1970. *Z. Tierpsychol.*, **27**: 303-351.—This paper, based on releases of more than 1,000 inexperienced pigeons, provides detailed confirmation of some ideas already familiar in the literature. The direction adopted by a pigeon on release appears to be a compromise between two tendencies—to fly towards home and to fly in a fixed direction characteristic of the home loft. The relative strength of the two tendencies depends on the distance from the loft, and also varies from loft to loft. Pigeons which had been allowed to fly freely around their lofts oriented no better than those raised in confinement, but many more of the former actually returned home: presumably this reflects their greater knowledge of landmarks.— I. C. T. Nisbet.

12. Further aviary experiments with homing pigeons: probable influence of dynamic factors of the atmosphere on their orientation. (Weitere Volierenversuche mit Brieftauben: wahrscheinlicher Einfluss dynamischer Faktoren der Atmosphäre auf die Orientierung.) Hans G. Wallraff. 1970. **Z.** *vergl. Physiol.,* **68:** 182-201. (German with English summary.)—It has long been known that there are factors connected with individual pigeon lofts which affect the orientation of birds raised in them. Wallraff has previously shown that pigeons raised in lofts surrounded by walls which exclude a view of the lower part of the sky cannot orient correctly. In this paper he shows that walls made of clear glass have the same effect: hence the information excluded by the walls cannot be simply visual. Walls made of nettle-cloth had the same effect.

When birds were raised in lofts with glass walls to north and south, their orientation was shifted in one direction; when birds were raised in lofts with glass walls to east and west, their orientation was shifted in the other direction. The critical experiment was to raise birds in lofts with the north and south walls solid and the east and west walls made of waved lamellae (louvres) which admitted air but blocked vision. The orientation of these birds was most similar to that of the birds without walls to east and west.

These results suggest that the birds were using orientation cues, learned in their home loft, which were carried by the air—not visual. Possible candidates are the wind and low frequency sound. These laborious experiments are a significant contribution to our knowledge of pigeon homing, but make it seem even more baffling.—I. C. T. Nisbet.

13. Wind and the direction of nocturnal songbird migration. S. A. Gauthreaux, jun. and K. P. Able. 1970. *Nature,* **228** (5270): 476-477.—Gauthreaux's portable ceilometer (*Bird-Banding,* **40** (4): 309, 1969) was used to study low-level passerine migration in Georgia and Louisiana on seven nights in spring and five in autumn. On each occasion the mean track of the birds was very close to the direction of the wind, as measured at midnight or 0600. On two nights when the wind changed the mean track of the birds changed correspondingly. The authors conclude that "at night songbird migrants in both spring and autumn tend to fly with the wind regardless of its direction". They suggest that radar observations of birds flying with cross-winds have generally been of high-flying non-passerines.

As they point out, acceptance of their conclusion would demand reevaluation of proposed mechanisms of orientation, so their evidence needs careful examination. In the first place, the agreement between their tracks and the wind direction is not much better than that reported in radar studies—the mean deviation being 19° for surface winds and 27° for winds at 305 m, with individual discrepancies up to 75°. Second, other interpretations of the behavior of migrants predict the same agreement. Several radar studies have reported different groups of echoes (identified with different species) moving consistently in different directions; several studies have reported that birds tend to migrate in greatest numbers when winds are generally favorable. Then, when the wind falls between the directions favored by two groups, both are likely to move in similar numbers, and the mean track is expected to be close to the direction of the wind. Third, the ceilometer technique, like moon-watching, provides biased information about flight directions. Birds flying with the wind have a higher ground-speed than those flying against or across it, and hence are seen in proportionately greater numbers. Before accepting Gauthreaux and Able's conclusions, we need to know whether the distribution of flight directions was always unimodal after correcting for this bias, and we need many more than 12 nights' data. Other cogent criticisms of this paper have been made by P. R. Evans (*Nature,* **228:** 1121, 1970).—I. C. T. Nisbet.

14. Ecology of the Great Reed Warbler, *Acrocephalus arundinaceus* (L.) wintering in the southern Congo savannah. A. de Roo and J. Deheeger. 1969. *Gerfaut,* **59** (2): 260-275.—A number of Great Reed Warblers banded in winter quarters near 6°S returned to the same place in subsequent winters. The birds are sedentary for much of the winter, but some move to well-watered places during the short dry season. Most do not reach the winter quarters until late November, although they leave Europe in August and September. Adults molt in Africa north of the equator; first-winter birds do not molt; some one-year old birds overshoot the molting-grounds of the adults and molt in winter quarters.—I. C. T. Nisbet.

15. Abnormal migration and hybridization of *Larus argentatus* and *L. fuscus* after interspecies fostering experiments. M. P. Harris. 1970. *Ibis,* **112** (4): 488-498.—The Herring and Lesser Black-backed Gulls are very closely-related species (often considered to be the ends of a "ring" of mutually intergrading forms) which breed in mixed colonies in western Europe, but which very rarely interbreed in the wild. Between 1962 and 1966 eggs of the two species were interchanged in a Welsh colony and almost 900 young were reared by the wrong species.

Normally British Herring Gulls do not migrate; most Lesser Black-backs migrate south to winter in Portugal and Africa. The cross-fostered Herring Gull chicks migrated to France, Spain and Portugal, but did not migrate as far as normal Lesser Black-back chicks. This might suggest that they had followed their foster parents during part of their migration. However, the cross-fostered Lesser Black-backs migrated to the normal wintering area for the population.

When the cross-fostered chicks returned as adults, many of them formed interspecies pairs, either with each other or with normally-raised birds. Others, however, mated with their own species. It appeared that females will usually only mate with males of their own species, or in the case of the cross-fostered birds, with males of their foster species. Males will mate with either species.—I. C. T. Nisbet.

16. Goose of the Himalayas. Lawrence W. Swan. 1970. *Nat. Hist.,* **79** (10): 68-75.—A flock of Bar-headed Geese (*Anser indicus*) was heard migrating north directly over the summit of Makalu, 8,480 m (27,824 feet) above sea level, although a nearby gorge might have afforded a passage at no more than 3000 m. There are other reports of the species from the vicinity of the highest Himalayas. The author speculates that the birds use the highest massifs as landmarks, and that flight across the mountain chain at lower altitudes might be dangerous because of extreme turbulence around the summits.—I. C. T. Nisbet.

17. Hurricane "Gladys" and its ornithological effect on the Maritime Provinces. Eric L. Mills. 1969. *Nova Scotia Bird Soc. NewsL.,* **11** (1): 6-16. —Hundreds of Laughing Gulls *Larus atricilla* and Black Skimmers *Rhynchops nigra,* together with a few rare southern terns, were brought to Nova Scotia by hurricane "Gladys" in late October 1968. The skimmers trickled southwards along the coast of New England and Long Island throughout November, but the Laughing Gulls disappeared abruptly.—I. C. T. Nisbet.

POPULATION DYNAMICS

(See 5, 34, 35, 36, 37)

NESTING AND REPRODUCTION

(See also 15, 42, 54, 82)

18. Observations on the breeding behaviour of the Diving Petrel *Pelecanoides u. urinatrix* (Gmelin). A. C. Thoresen. 1969. *Notornis,* **16:** 241-260.—A reproduction study in the usual format for procellariiform work: vocalizations, pre-egg stage, egg stage, chick development, feeding of the chick. Also included are sections on chick temperature and mortality, nest sanitation, and external parasites including a flea, *Parapsyllus jacksoni,* for which the diving petrel seems to be a new host.

His previous research on the Cassin Auklet, *Ptychoramphus aleutica,* enables the author to compare its ecology and behavior with those of the convergent *P. u. urinatrix.*—Thomas C. Grubb, Jr.

19. The breeding biology of the Pintailed Sandgrouse in captivity. (Zur Brutbiologie und Zucht des Spiessflughuhns (*Pterocles alchata*) in Gefangenschaft.) O. von Frisch. 1970. *J. Orn.,* **111** (2): 189-195.—In my "Development of Behavior in Precocial Birds" (1962, *Linn. Soc. N. Y. Trans.,* **8**), due to lack of detailed information, I mistakenly classified the Sandgrouse (Pteroclidae) as belonging to "Precocials 2," where the newly-hatched chicks follow their parents but find their own food. The careful studies of Otto von Frisch have shown that they belong to Precocials 3 where the chicks follow the parents but must be shown food. Indeed, this picking up and dropping of dry seeds before the chicks would seem to be essential for proper nourishment of the young for many days.—Margaret M. Nice.

20. On the importance of nest illumination to cavity-nesting birds. (O znachenii osvechennosti gnezda u ptits-duplognezdinikov.) K. N. Blagosklonov. 1970. *Byull. moskovskogo obschsh. isp. priorody, otdel. biol.*, **75** (3): 45-47. (In Russian, English summary.)—The occupation of over 1,000 wooden board nestboxes, suspended in 800 hectares of forest, chiefly by various species of chickadees, redstarts and Pied flycatchers, was observed over the course of 17 years, in the Moscow region. Occupation of individual boxes diminished from the 3rd year onward, apparently owing to gradual darkening of the wood. Old nests were reoccupied after inner walls were whitewashed; new nests artificially blackened inside were usually unoccupied.—Leon Kelso.

21. Natural history of the avian families Dendrocolaptidae (Woodhewers) and Furnariidae (Ovenbirds). J. A. Feduccia. 1970. *J. Grad. Res. Center, South. Meth. Univ.*, **38**: 1-26.—Reviews the literature of these two families with emphasis on habitat, food, foraging behavior, and nidification. Discussions of these topics are presented and organized for each of the different subfamilies.—Joel Cracraft.

22. Orientation and shifting of eggs in nests as factors in avian embryo development. (Orientatsiya i peremeshchenie yaits v gnezdakh kak faktory embrionalnogo razvitiya ptits.) A. Bolotnikov, Y. Kamenskii, L. Afanaseva, and L. Yakonenko. 1970. *Ekologiya*, **1** (5): 85-87.—In 77 nests of 11 passerine species, containing 349 eggs, 84% of the eggs were positioned with large end upward or parallel to the periphery. The apparent effect is more ample contact with the sitter's warming surface. If randomly arranged by an observer the sitter rearranges the clutch to suit. If eggs are unmoved the embryos die owing to adhesion to the shell membrane. Rotation and shifting of eggs (over 20 times per day in the few cases surveillanced by actograph) provides more equable warming, varies temperature gradient in eggs, and facilitates change of air in nest, and respiration through the egg shell, all of which are deemed necessary for natural incubation.—Leon Kelso.

23. The effect of microwave radiation on the growth and reproduction of chickens. M. A. K. Hamid, R. J. Boulanger, G. C. Hoodgson, P. A. Kondra, K. Smith, and D. B. Bragg. 1969. *J. microwave power*, **4** (4): 253-256.—In tests for practical application no definite harmful and slight beneficial aftereffects were noted under experimental conditions.—Leon Kelso.

24. Conditions and nature of avian embryo development during egg deposition. (Usloviya i kharakter razvitiya embrionov ptits v period yaitsekladki.) A. Bolotnikov, A. Shurakov Yu. Kamenskii, and V. Koroleev. 1970. *Ekologiya*, **1** (4): 40-46. (In Russian.) Bibliography of 18 titles.—Of 46 bird species studied, including galliformes, anseriformes and passerines, it was found that embryo development actually starts at deposition of the first egg, with or without continuous incubation. For the Common Teal, *Anas crecca*, numerous details of the first egg's embryo growth through 8 days during completion of the clutch are given. Experimental delays of 20-25 days before start of incubation reduced hatchability of clutches from 3.2% to zero.—Leon Kelso.

BEHAVIOR

(See also 12, 18, 19, 20, 21, 22, 30, 31, 48, 49, 83, 86, 91)

25. The historical origins of "ethology" and "comparative psychology." J. Jaynes. 1969. *Anim. Behav.*, **17:** 601-606.—The terms are traced back to the debates of early nineteenth century French biology, where the issue was over the value of field studies versus laboratory anatomy. The paper concludes: "The unknown tyranny of the past is the source of all unreason."—Jack P. Hailman.

26. Observational learning by Fork-tailed Flycatchers (*Muscivora tyrannus*). J. Alcock. 1969. *Anim. Behav.*, **17:** 652-657.—Flycatchers were given a distasteful butterfly, and if failing to eat any of three were then allowed to watch a conspecific eat two imperfect mimic species (or fail to eat them, in control groups). When tested alone, the observers ate the mimics more often if they had seen a conspecific do it than if the other bird has avoided the mimics during the observational time. The difference is not large, and most of the birds still avoided the mimics, but the phenomenon seems real.—Jack P. Hailman.

27. Motivationally controlled stimulus preferences in chicks of the Black-headed Gull (*Larus ridibundus* L.). M. Impekoven. 1969. *Anim. Behav.*, **17:** 252-270.—Insufficient attention has been paid to the possible effects of motivational states upon preferences animals show among stimuli. In this detailed and complex study, Monica Impekoven studied the effects of hunger and temperature on pecking and approaching the parent to be brooded. Basically, she feels that the data indicate that motivational factors influence responses to stimuli mainly by altering general activity, rather then by specifically altering stimulus-filtering mechanisms.—Jack P. Hailman.

28. Roosting of Starlings (*Sturnus vulgaris*): a function of light and time. G. J. Davis and J. F. Lussenhop. 1970. *Anim. Behav.*, **18:** 362-365.—The time at which birds gather at the roost in the evening is loosely correlated with the light intensity, as shown by many studies on different roosting species. Davis and Lussenhop show that by taking into account both the minutes before sunset and the light intensity under different conditions one achieves a more predictive flocking time. Actual data points are presented, so the reader can judge the variation yet to be explained. The mechanisms for sensing this time-intensity relationship are obscure.—Jack P. Hailman.

ECOLOGY

(See also 18, 19, 20, 21, 32, 34, 39, 57, 58, 60, 61, 82, 88, 90)

29. Introduction. D. Lack. 1970. *Symp. Brit. Ecol. Soc.*, **10:** xiii-xx.—This is, as its title suggests, the introduction to the tenth symposium of the British Ecological Society, which was held in Aberdeen in March, 1969. The symposium was entitled, "Animal populations in relation to their food resources", and not surprisingly, David Lack opened the proceedings. In this short seven-page introduction, Lack draws heavily upon birds for examples. He again outlines his thesis, most clearly laid out in 1954, that each species of animal reproduces as rapidly as possible, and that its numbers are limited by a consequent density-dependent death-rate. He stresses the need for additional information on the behavior involved in food selection and the roles played by hereditary factors and learning in habitat utilization and food selection. Lastly he points out how research in this area may throw light on problems of practical interest to man. Careful studies, such as those by Murton on Wood Pigeons (*Columba palumbus*) and by Norton-Griffiths on the Oystercatcher (*Haematopus ostralegus*), indicate that the simplest solution to control of a species deemed to be a "pest" may not be through simply shooting it.

Thus he opens the symposium, which devotes itself to a topic that he was in large part responsible for generating interest in. Six out of the 22 papers (generally rather long) devote themselves to birds, and parts of several others utilize avian examples extensively. This symposium is clearly an important contribution to an area of considerable controversy.—Douglass H. Morse.

30. The ecology of the Song-thrush (*Turdus philomelos* Br.) and Blackbird (*Turdus merula* L.) during the breeding season in an area of their joint occurrence. A. Dyrcz. 1969. *Ekol. Polska* (Ser. A), **17**: 735-793.—One of Darwin's examples of interspecific competition (On the origin of species, 1859) dealt with the Song Thrush, which at that time was apparently being displaced from parts of Scotland by another congener, the Mistle Thrush (*Turdus viscivorus*). The present study documents the apparent ecological displacement of the Song Thrush by its congener, the (European) Blackbird. In Poland the two species have extremely similar ecological requirements, so similar, in fact, that Dyrcz believes that the situation is rapidly changing (in favor of the Blackbird). The slight warming of Europe over the past 200 years may have favored the spread of the latter species northward into new areas, as may have the increasing modification of the habitat by man. Song Thrushes are more abundant than Blackbirds in wooded areas, while the opposite situation holds about human habitation. Drycz believes that the main factor in limiting supply is nesting places. Blackbirds accept a wider variety of nesting places than Song Thrushes and have a higher nesting success.

Potentially competing species pairs often exhibit interspecific territoriality, but this does not seem to be the case between these two species. In fact, they often nest closely to each other. Relatively few instances of interspecific aggression were noted, and it would thus appear that if competition is indeed occurring, it is directly for the limiting factor, which as indicated above, Dyrcz believes to be nesting space. However, among the few cases of interspecific aggression noted were cases of Blackbirds dispossessing Song Thrushes from their nest sites.

Dyrcz does not believe that the usually-mentioned limited factor, food, is the critical one in this case. Food obtained by the two species showed great similarities, though Song Thrushes concentrated somewhat more upon snails and slugs, while Blackbirds fed more heavily upon beetles. Information was based upon food given to young by adults, and the author discusses why he feels that it serves as a reasonably representative sample of the adults' fare as well. Data are mentioned, however, which indicate that weights of late broods were lighter than those of earlier ones, which suggests that the food supply may play a role in the breeding biology of these species. Also, food items of the two species in the poorest habitats showed the greatest differences, and the food items utilized in the poorest habitats also were the most varied of all for both species. While these factors in themselves do not indicate that food is a limiting factor in some cases, they suggest that it would be premature to eliminate this item as a possible limiting factor, at least in some of the habitats utilized.

In short, a strong case is made for nesting sites being the major limiting factor of the populations; however, the possibility of food limitation is not completely eliminated, even though over one-third of the paper is devoted to this topic. Regardless of one's conclusions, there are lots of seldom-available data presented in this paper, making it a valuable source of information.—Douglass H. Morse.

31. The Ecology and Behavior of the Lewis Woodpecker (*Asyndesmus lewis*). C. E. Bock. 1970. *U. Calif. publ. zool.*, **92**: 1:100. 6 plates, bibliography of 217 titles. $3.00.—This doctoral research on a very distinctive American species, long overdue a monograph, deserves rather more than 100 pages. The extensive field work, mostly Californian, covers the usual topics, of which may be examined the more neglected one of food storage. This was observed and is discussed in considerable detail. Since not only storage but considerable trading about or restorage was found, it would not have been malaprop to at least mention the parallel instances in the Eurasian Nutcracker (*Nucifraga caryocatactes*) and in various species of Paridae, or to recall its oft-noted resemblance in aspect and habits to various Corvidae.—Leon Kelso.

WILDLIFE MANAGEMENT AND ECONOMIC ORNITHOLOGY

(See also 34, 91)

32. ***Proceedings of the World Conference on Bird Hazards to Aircraft,*** Queen's University, Kingston, Ontario, Canada, 2-5 September 1969. Ed. M. S. Kuhring. vii + 542 pp. Ottawa: National Research Council of Canada. —Collisions of birds with aircraft are an expensive nuisance, which probably cost the airlines and air forces of the world some tens of millions—perhaps more than 100 million—dollars every year in engine repairs and 'down time'. Accidents which are fatal or near-fatal (to men) are infrequent, but are not negligible, and are probably becoming increasingly probable as new aircraft are introduced. Reading carefully between the lines of the papers and discussion of this inter-disciplinary conference, one can make certain generalizations: airport managers are concerned about the problem but have little money or power to solve it; bureaucrats lack the motivation or the ability to tackle a complex problem for which the responsibility can be passed on to each other; airlines do not want the dangers publicized; airframe and engine manufacturers (who make money from repairs) do not want the problem solved too efficiently. Ornithologists in general are not genuinely interested in the problem, but look upon it hopefully as a source of funds for studies in bioacoustics, radar and migration.

Of the 21 countries represented at the conference, only the host, Canada, seems to have appreciated fully that a complex problem requires a many-faceted solution, has put together a multi-disciplinary team of competent engineers, planners and biologists, and has achieved real progress in reducing damage. The West Germans have an energetic program of mapping migration routes and modifying airports, but it is not clear that they are doing either in the right way: their "ecological" program is most notable for its reliance on heavy use of toxic chemicals. Australia, Britain, France and the Netherlands have somewhat more than token programs: that of the United States, in view of her pre-eminence in world aviation, is startling for its parsimony and ineffectiveness.

Several papers on radar and migration were reviewed in the last issue of *Bird-Banding.* There were two papers on the ecology of gulls and one on roosting behavior of crows, but these were in a sense uninvited because the organizers did not include ecology as a topic in the prospectus for the conference. (Similarly, the ecology of aircraft was completely omitted, although one would have thought it central to the problem.) A much-publicized proposal to knock down birds with microwaves turned out to involve dangerous and impractical power levels. Of two papers on falconry, one experiment with Goshawks had mixed success, but a British team claimed complete success in eliminating bird strikes from a Navy airfield by intensive flying of Peregrines. With a consumption of two Peregrines per year, they reduced damage from $600,000 per year to zero. Quote that when a pesticide promoter says that a Peregrine has no economic value!

The highlight of the conference was a paper by G. W. Schaefer, predicting greatly increased rates of dangerous collisions with the new jumbo jets, because of their huge engine intakes. The paper engendered intense controversy, and has been discreetly omitted from the Proceedings and replaced by an innocuous proposal to detect flocks of gulls on runways. However, the preprint looks convincing. Pending clarification, the best advice an ornithologist can offer a prospective passenger is not to fly on a 747 from a coastal airport. The other moral is: don't trust engineers, bureaucrats or biologists to attend to your safety.—I. C. T. Nisbet.

33. **Canadian settlers.** (Kanadensiska nybyggare.) E. Fabricius. 1970. *Zoologiska revy,* **32** (1): 19-25.—Introduced into Sweden by Bengt Berg, the Canada Goose, *Branta canadensis,* began nesting there in 1933, at Kalmarsund, and now comprises a large population in numerous lakes and tarns, in close association with the Common Eider, and forms some mixed colonies with the Greylag Goose, *Anser anser.* It is believed that the American newcomer exerts a protective influence for a present increase of Eiders on Blekinge Archipelago.—Leon Kelso.

CONSERVATION AND ENVIRONMENTAL QUALITY

(See also 32)

34. Peregrine Falcons, pollutants, and propaganda. J. J. Hickey. 1970. *Canad. Field Nat.*, **84:** 207-208.—This editorial introduces a "Special Issue, dedicated to the survival of the Peregrine Falcon." Hickey points out that *Falco peregrinus* has received more scientific and other attention in the last eight years than in the previous century—thanks due in part to Hickey himself, I might add. Huskins is quoted as having distinguished three steps in the reaction of agriculturists to ecological findings: (1) We do not believe it; (2) It is of no importance anyways; and (3) We knew it all the time. Hickey thinks we are presently receiving stage 2 reactions with regard to organopesticides, and documents the fallacious arguments being used by the National Agricultural Chemicals Association. Hickey makes a point here that is badly in need of circulation: too many scientists are speaking on ecological topics about which they know really very little, and thus jeopardizing the road to truth and sound ecological management: "We are beginning to suffer from too many instant-ecologists, instant-entomologists, instant-oceonographers, and the like." Assays of dieldrin, mercury and polychlorinated biphenyls in the environment point to an extremely complex pollution. "This is no place for Analysis by the Averted Glance."—Jack P. Hailman.

35. Observations on the decline and survival of the Peregrine Falcon. R. W. Nelson. 1970. *Canad. Field Nat.*, **84:** 313-319.—Wayne Nelson subjects the problem to close scrutiny, particularly with regard to opinions of "strict protectionists" that have appeared in *CFN* recently. He lists factors about Peregrine biology that should be kept in mind when discussing the problem of the species' survival: (1) a non-breeding surplus of adult falcons; (2) the concentration of mortality during the first winter; (3) the apparent maintenance of some regional populations, especially those along the coast of British Columbia; (4) the apparently small populational effects of harvesting young falcons, if controlled; (5) the contribution of falconists in gathering basic information about falcon biology; (6) the success (admittedly limited thus far) of breeding falcons in captivity; (7) the fact that *timing* of nest visits in the crucial factor in whether or not birds desert; and (8) the regulatory laws and resolutions of societies with regard to falcon protection. From these considerations, Nelson argues persuasively that in parallel with attempts to preserve and increase wild Peregrines, a program of breeding them in captivity should be initiated. This latter could be the only hope of saving the species.

There comes a time when every reviewer must (or at least should) eat his words—even as crusty a reviewer as I. My review of Mosquin's "Toward legislation to protect Peregrine Falcons" (*Bird-Banding*, **41:** 252, 1970, no. **29**) stated that NAFA is "vigorously fighting laws protecting falcons," and this simply is untrue. Mosquin quotes individuals who are neither official spokesmen of NAFA in these matters, nor representative members. I drew the conclusion that they were these things from the fact that they formerly held high offices in NAFA, and from the general context of Mosquin's editorial. It is for the reader to decide whether my perceptive powers are dull, or whether any reasonable man would have drawn the same conclusions from Mosquin's article.

Many more facts are now at my disposal. The resolutions passed at the 1969 NAFA Peregrine Symposium and other material that has come to my attention make it clear to me that the fight to save the peregrine cannot succeed without the help of the falconers and their organizations. It seems to me that they deserve the help and cooperation of us all.—Jack P. Hailman.

36. The North American Peregrine Survey, 1970. T. J. Cade and R. Fyfe (eds). 1970. *Canad. Field Nat.*, **84:** 231-245.—This is a collection of reports by a large number of investigators in various parts of Canada and Alaska, including about 237 known eyries. Eggshells are 15-20% thinner than before 1947, thickness being negatively correlated with chlorinated hydrocarbon residue in egg contents and adult tissues. "And so the Peregrine continues to disappear at a rate that could bring it to extinction in North America in this decade."—Jack P. Hailman.

37. Organochlorine pollutants in Peregrines and Merlins migrating through Wisconsin. R. W. Risebrough, G. L. Florant and D. D. Berger. 1970. *Canad. Field Nat.*, **84:** 247-253.—Ten *Falco peregrinus* and seven *F. columbarius* were subjected to biopsy, analysis being conducted for DDE, p, p'-DDD, p, p'-DDT, total DDT and PCB. Levels in Peregrines were about the same as previous determinations made by WARF, Inc., although PCB's were surprizingly high (not separated out in previous assays). Merlins showed almost the same DDE levels as Peregrines, and may therefore be suffering from eggshell thinning. Comparison of various pollutants suggests that the Merlins and Peregrines that migrate through Wisconsin are coming from different Canadian areas—a macabre new technique for zoogeographic study if ever I read one.

The evidence now suggests that PCB's, although correlated significantly with population declines, are not having their effects through eggshell thinning, which is more highly correlated with DDE contamination. Possibly PCB causes delayed breeding, a serious factor in the lives of arctic birds that have a restricted nesting cycle. The possibility that a new, highly toxic contaminant may be a factor in the complex pollution story is raised: watch for reports of chlorinated dibenzofurans.—Jack P. Hailman.

38. Organochlorine residues in Alaskan Peregrine Falcons (*Falco peregrinus* Tunstall), Rough-legged Hawks (*Buteo lagopus* Pontoppidan) and their prey. J. L. Lincer, T. J. Cade and J. M. Devine. 1970. *Canad. Field Nat.*, **84**: 255-263.—Very briefly, the Roughlegs contained lowered levels than Peregrines (up to about 90% in brain and muscle), which fact reflects the less contaminated prey upon which the former feeds.—Jack P. Hailman.

39. Mercury contamination of Canadian prairie seed eaters and their avian predators. N. Fimreite, R. W. Fyfe and J. A. Keith. 1970. *Canad. Field Nat.*, **84**: 269-276.—A huge number of samples of prairie seed eaters were taken to compare those from fields treated with mercury-containing dressings with those from untreated fields. Comparions of liver assays show mercury contamination to be widespread in the prairie wildlife (birds and mammals). Mercury levels are higher in small birds than small mammals, and this fact is reflected in high mercury contamination of avian-feeding predators (falcons and accipiter) compared with mammalian-feeding predators (eagles, buteos and harriers). Since recent evidence shows mercury contamination retards hatchability in pheasant eggs, mercury may be contributing to the decline in populations of large avian predators.—Jack P. Hailman.

40. Shell thinning in eggs of Ungava Peregrines. D. D. Berger, D. W. Anderson, J. D. Weaver and R. W. Risebrough. 1970. *Canad. Field Nat.*, **84**: 265-267.—The data indicate "that the population has reached a critical level of shell thinning resulting in the production of broken or cracked eggs and lower than normal numbers of young hatched."—Jack P. Hailman.

41. Radioactive cesium accumulation in terrestrial vertebrate populations. (Nakoplenie radioaktivnogo tseziya v popylatsiyakh nazemnykh pozvonochnyk.) A. I. Ilenko and E. A. Fedorov. 1970. *Z. zhurn.*, **49**(9): 1370-1376. (In Russian, with English summary.)—Cesium-137 is a common radioactive isotope waste emanating from atomic energy installations. In test areas artificially contaminated by Cs137 in aerosols in densities of 4-8 microcuries per square meter, its accumulation in various vertebrates of the major classes, including, of birds: Black Grouse (*Lyrurus tetrix*), Magpie (*Pica pica*), Starling (*Sturnus vulgaris*), Tree Sparrow (*Passer montanus*), Tree Pipit (*Anthus trivialis*), Yellowhammer (*Emberiza citrinella*), and Tawny Owl (*Strix aluco*), was ascertained. Exposure was for periods of 6, 14, and 18 months. In general accumulation per weight was higher in breeding females, higher in small species of animals than in large ones, in young than in adults. There is a bibliography of 25 titles, and a review of American work.—Leon Kelso.

42. Great moments in action: the story of the Sun Life falcons. G. Harper Hall. 1970. *Canad. Field Nat.*, **84**: 209-230.—Reprinted here is Hall's 1955 "modern classic" about peregrines nesting on the Sun Life Building in Montreal, complete with a preface to the reprint by T. Mosquin, and a short biography of Hall (who died in a 1958 auto accident) by G. H. Montgomery. In 1949 Hall noticed that a nesting female ate one of her eggs, and that observation spurred a close and careful watch over the birds for years to come. Hall's records document breeding success from 1940 until 1952. *CFN* has done a service by making Hall's little study available to us all.—Jack P. Hailman.

43. A simple device for measuring eggshell thickness. V. Lewin. 1970 *Canad. Field Nat.*, **84:** 305.—Basically, a ball-bearing is inserted into a micrometer. Should be a useful aid to those studying pesticide effects on eggshell thickness.—Jack P. Hailman.

PARASITES AND DISEASES

(See 18)

PHYSIOLOGY AND PSYCHOLOGY

(**See also 8, 9, 16, 22, 23, 24, 25, 26, 38, 39, 40, 41, 63, 68, 99**)

44. Absorption spectra and function of the colored oil drops in the pigeon retina. P. King-Smith. 1969. *Vision res.*, **9** (11): 1391-1399. Bibliography of 19 titles.—The results of this microchemical and microelectronic study, so the writer states, support the observations of Hailman (1964. *Nature*, **204:** 710-712) that relation of gull pecking behavior to monochromatic light can be related to a single pigment theory; i.e. that while two visual pigments are present in the pigeon eye retina, only one is functional in the oil droplets signaling response to any particular color.—Leon Kelso.

45. The visual pigments of several species of birds. A Sillman. 1969. *Vision res.*, **9** (9): 1063-1077.—Twenty avian species of 8 orders and 11 families were examined; besides rhodopsin, some species had a second, anomalous, pigment in the retina, showing no correlation to systematic position of the birds. The Poor-will and Burrowing Owl retinas showed the most pronounced visual pigmentation; none could be found in the eye of the Emerald Toucanet, *Aulacorhynchus prasina.*—Leon Kelso.

46. The monocular eye movements of the pigeon. P. Nye. 1969. *Vision Res.*, **9** (1): 133-144.—Flick, drift, tremor, and oscillation movements were identified. "The observations lead to the conclusion that although the range of rotational motion available to the pigeon is less than in man it nevertheless possesses a well developed and precisely controlled oculomotor system which appears in some respects to out-perform our own."—Leon Kelso.

47. Further investigations on the structure and innervation of the pineal organ of *Passer domesticus* L. (Weitere Untersuchungen zur Feinstruktur und Innervation des Pinealorgans von *Passer domesticus* L.) M. Ueck. 1970. *Z. Zellforschung*, **105** (2): 276-302. Bibliography of 52 titles. (In German, English summary.)—After scrutiny through electron miscroscopic magnification: "There is no evidence found in the material of this study to suggest that sympathetic nerve fibers perforate the basement membrane and enter the parenchymal cell complexes of the pineal organ." The role of the pineal in "photo-neuro-endocrine" regulation is still uncertain.—Leon Kelso.

48. On the memory problem. (K probleme pamyati.) N. L. Krushinskaya. 1970. *Priroda*, **1970** (9): 75-78. (In Russian.)—The problem is pursued through experiments and analysis of the performance of Nutcrackers, *Nucifraga caryocatactes*, locating their food caches in artificial conditions; 4 normal control individuals recovered 104 out of 134 caches (78%); 3 with neostriatum tissue of brain removed recovered 49 of 54 (91%); 6 with hippocampal cortex removed, only 18 of 143 caches (13%).—Leon Kelso.

[The temptation to calculate statistics was too much to overcome, and I found by Chi-square analysis that birds lacking neostriatum *do* in fact find caches better than the controls, at $p < .05$. Draw your own conclusions.—*Review Editor*.]

49. Passive localization in owls. (Passivnaya lokatsiya sov.) V. D. Ilichev. 1970. *Uspekhi sovremennoi biologii*, **70** (4): 120-136. (In Russian.)—A review of this author's extensive work on the subject, summarizing also American and European studies, with a bibliography of 35 titles. The preciseness of passive localization in owls correlates with such structural and functional features of the auditory system as: prominent aural folds and operculi, asymmetry of outer ears, enlargement of tympanic membrane with lateral shift of extracolumellar attachment to it, development of umbo clypeoli, elongation of basilar membrane, increased number of auditory nerves, and complexity of auditory nuclei of medulla oblongata. These features increase developmentally in an eco-morphologic series paralleling owl species' position on diurnal to nocturnal, and insectivorous to carnivorous scales. Carnivorousness in nocturnal life requires the highest exactitude for passive localization of prey.—Leon Kelso.

50. Energy expenditure for free flight by the Purple Martin (*Progne subis*). J. M. Utter and E. A. LeFebvre. 1970. *Comp. Biochem. Physiol.*, **35:** 713-719.—This study investigated the relative amounts of energy expended in flight compared to standard metabolic expenditure. The increase in energy during flight was from 4.6 to 6.1 times as great. Compilation of data from the literature suggests that the larger the bird the greater the energy cost of flight. Nonpasserines appear to have a larger ratio of flight energy to standard metabolic expenditure than passerines. If true, and more data need to be collected, this difference may have been an important factor in the origin and diversification of passerines. —Joel Cracraft.

51. *The Biological Clock (Two Views).* F. A. Brown, J. W. Hastings, and J. D. Palmer. 1970. Academic Press. New York and London. VIII - 94 pp. 59 figures, bibliography of 29 titles. $1.95.—Following an introduction by Palmer, Brown contributes the "Hypothesis of environmental timing of the clock", which elaborates the theme of his article reviewed in *Bird-Banding*, 1970, **41** (3): 253 Opposing this, Hastings describes the "Cellular-biochemical clock hypothesis", of built-in, endogenous motivation and control. We have here another phase of the environment versus heredity debate. Not arguing about either, we note here a third element entering scientific life and publications in addition to personnel management, journalism, and perhaps others, i.e. what may be called "creative typography" anent its becoming a requirement. For example printing and illustration come in black and what is approximately brick-red. Some of the figures, most of which are graphs, are in black, others — red, and others red and black; the "contents" (black) are printed in red; the lettering on the cover involves four colors. This may or may not be based on theory of psychological impact. All in all the text is a good buy for up-to-dateness and not taking most of a five-dollar bill.—Leon Kelso.

52. Electrical activity from the pectoral muscles of a flying bird, the budgerigar. A. Aulie. 1970. *Comp. biochem. physiol.*, **36** (2): 297-300.—Several *Melopsittacus undulatus* were trained to perform in a wind tunnel. Electronic preamplifier (Grass P9B) and storage oscilloscope (Tektronix 564) detected and recorded electrical impulses. Wingbeat frequency of the pectorals was 16 per second, scarcely varying over a wide range of flight speeds. "Twitch" contractions characterized "normal" flight; and were replaced by short "tetanic" contractions druing strenuous flight. The mean muscle "potential amplitude" was lowest at 35 km per hour, but rose higher for velocities faster or slower than this.—Leon Kelso.

53. Timing mechanisms and the physiology of bird migration. P. Evans. 1970. *Sci. progress, Oxford*, **58** (230): 263-275. Bibliography of 31 titles.—A review of the various theories on the subject, concluding that an internal "clock" may be responsible for timing the migration of species that cross the equator, and of all species in autumn, but that until experiments in year-long continuous light and dark show persistent endogenous rhythms their existence cannot be considered fully proven; that bird migration probably evolved several times independently, so several timing systems may exist.—Leon Kelso.

54. Why don't Bobolinks breed in Brazil? W. M. Hamner and J. Stocking. 1970. *Ecology,* **51** (4): 743-751.—When attempting to account for the initiation of migration by using photoperiodic information, investigators have always faced the thorny problem of accounting for trans-equatorial migrants. If most species (those wintering in the hemisphere in which they breed) respond at appropriate times to short day length and long day length, then the trans-equatorial migrants obtain an entire set of spurious data. To date three explanations have been given to attempt to reconcile this problem: (1) that these birds are physiologically refractory to photostimulation when daylengths are increasing in the opposite hemisphere, (2) that they have a very slow rate of response following termination of refractoriness, or (3) that an endogenous annual rhythm is the major component of the system and is being synchronized by a variety of environmental stimuli. Using data obtained from a rather long-term laboratory experiment of Bobolinks (*Dolichonyx oryzivorus*) the authors attempt to evaluate these hypotheses. Their results are rather equivocal, but they tentatively conclude that while there is no support for either of the first two explanations in the Bobolink, there is some support for an endogenous timing component operating in these birds. This contention is based upon a spontaneous increase in body weight at an appropriate time of the year in birds under continuous illumination and upon three (out of 15) birds in this group showing spontaneous testicular development (but several months early). Clearly, the results of this set of experiments are somewhat ambiguous, and the conclusions should be accepted with considerable caution (which the authors do). They close by saying that photoperiodic mechanisms are extremely complex, more so than previously suspected. The reviewer would agree to this statement on the basis of this paper and looks forward to further elucidation of this phenomenon by the writers.—Douglass H. Morse.

55. Respiration, oxygen consumption and heart rate in some birds during rest and flight. M. Berger, J. S. Hart and O. Z. Roy. 1970. *Z. vergl. Physiol.,* **66:** 201-214.—Metabolism in Evening Grosbeaks *Hesperiphona vespertina,* Ringbilled Gulls *Larus delawarensis* and Black Ducks *Anas rubripes* during short flights (7-15 sec.) was estimated by equipping them with rubber masks and measuring the breathing frequency, tidal volume and oxygen content of the expired air. The first two quantities increased considerably on taking flight, but the last changed only a little. The total oxygen consumption in flight was about 10 times the standard (basal) rate in all species, regardless of size. Heart rates during and after flight, measured in 12 species, were 2-4 times those measured at rest.

No information is given about the speed of flight in these experiments. It would be valuable to estimate the aerodynamic drag on the tubes carried by the birds: this would presumably be relatively more important for the smaller birds.—I. C. T. Nisbet.

56. The structure of pigeon muscle and its changes due to tenotomy. R. S. Hikida and W. J. Bock. 1970. *J. Exp. Zool.,* **175:** 343-356.—After severing the tendon of the biventer cervicis it was found that the fast (fibrillenstruktur) fibers had a more rapid onset of atrophy than slow (felderstruktur) fibers, although the latter attained the same degree of atrophy in later stages.—Joel Cracraft.

57. Is an endozoic disposal of atropine by Blackbirds and Starlings possible? (Ist eine endozoisch Verbreitung der Tollkirsche durch Amsel und Star moglich?.) F. Seuter. 1970. *Zool. Jb., physiol.,* **75** (2): 342-359. (In German, English summary.)—Blackbirds' (*Turdus merula*) and Starlings' (*Sturnus vulgaris*) tolerance of atropine doses, up to 100 mg, exceeds that of humans by 10:1. Resorption from the intestine is very rapid, with elimination in 3.5 hrs. With no thorough explanation to date an old mystery still remains, indicating that avian physiological mechanisms are widely different from those of mammals. It is suggested that nightshade (*Atropa belladonna*) berries are not poisonous because 65% of the hyoscyamine is held within the seeds which are passed out of the intestine undigested.—Leon Kelso.

58. Excessive drinking *Polydipsia* in a Galapagos Mockingbird (*Nesomimus*). W. Dunson. 1970. *Comp. biochem. and phys.,* **36** (1): 143-151.

Observers have noted unusual thirst of mockingbirds on Espanola (Hood) Island. Preliminary observation by recording of trapped birds' drinking of distilled to highly saline water showed 54 to 119% of body weight consumption per day. Polydipsia also occurs in a mutant strain of chickens, 13 to 57% body wt. per day. —Leon Kelso.

59. Water conservation by inhibition of food intake. D. McFarland and P. Wright. 1969. *Physiology and behavior,* 4 (1): 95-99.—In experiments with the Barbary Dove (*Streptopelia risoria*) it was found that reduced food intake results in a marked decline in water intake and loss, and rectal temperature. Conversely, water deprivation reduces food intake.—Leon Kelso.

60. Seasonal changes of the caloric value and chemical composition of the body of the Partridge (*Perdix perdix* L.). M. M. Szwykowska. 1969. *Ekol. Polska,* Ser. A. **17** (40): 795-809.—During the past two decades or so ecologists have been bemused with a new plaything, the bomb calorimeter, which allows them to determine the caloric values of animal tissues, among other things. Lists of value from different species have been published, and attempts made to account for the differences obtained. In spite of this, in few cases have detailed studies been undertaken on birds over a period of time, and most of these have dealt with migratory species. Since changes in caloric value of animal tissues are caused principally by the stored fat content and the water content in the tissues, one might anticipate that changes would occur in the yearly cycle of an organism. In this study adult Partridges were analyzed throughout the period of a year.

The caloric value of Partridge biomass (wet weight) changes markedly during the period of a season, being highest during the winter, decreasing in the spring, and reaching a low point in the summer and early fall. Caloric values of dry weight vary in a similar manner, differences being caused primarily by the changes in fat content. Water content is inversely related to fat content. These differences coincide with problems encountered by the birds at particular seasons. The high caloric values accompany the putting on of fat during the winter season. This decreased with the coming of the spring, though there was a slight upsurge during May as a result of the high-caloric yolk in females' ovaries. The low summer value is probably the result of the molt period with its associated demands for feather growth. One might predict that non-migratory species would differ from the migratory species, which put on large amounts of fat in the spring.—Douglass H. Morse.

61. Variations in the body composition and caloric value of nestling Tree Sparrows (*Passer m. montanus* L.). A. Myrcha and J. Pinowski. 1969. *Bull. Acad. Polonaise Sci.,* **17** (8): 475-480.—The results from this study are readily comparable to those of Review **60**. Caloric value of nestlings increased up to the age of 11 days and then leveled off. This change depended primarily upon the water content of the tissues, a factor that overrode the fat content.—Douglass H. Morse.

MORPHOLOGY AND ANATOMY

(See also 43, 47, 49, 56, 84)

62. Avian functional anatomic problems. A. M. Lucas. 1970. *Federation proceedings,* **29** (5): 1641-1648. Bibliography of 43 titles.—Besides a detailed review of the usual current problems, this account ends with a hint of a breakaway from the traditional "dead and dry" concept of birds' integument. Stating that "Feathers receive their coating of oil in the preening process rather than by secretions from follicle cells." (for which we would ask, then why the difference between feather lipids and preen gland contents?), the author refers to his previous: Lipoid secretion in the avian epidermis. *Anatomical record,* 1968, **160**: 386, where evidence that the avian epidermis is secretory is presented, finding "secretory material" in the epidermal layers adjacent to the follicle cavity, and also in the basal layer of the follicle sheath epidermis.—Leon Kelso.

63. On the regio olfactoria and nervus olfactorius in the gull *Larus argentatus.* (Untersuchungen an Regio olfactoria und Nervus olfactorius der Silbermowe (*Larus argentatus*). D. Drenkhahn. 1970. *Z. Zellforsch. und Mikroscopanatomie,* **106** (1): 119-142. Bibliography of 59 titles; 13 figs. of up to 55,000x magnification.—[Per M. P. Harris. 1970. *Ibis,* **122** (4): 488.] The Herring Gull must be about the most thoroughly investigated of birds, and there is no disputing that in the multiplicity of ultramicroscopic details elaborated and beautifully illustrated in this study. The olfactory surface for example shows per vesicle unit 7-13 olfactory cilia and about 200 microvilli. Thus the sense of smell in birds may be much more developed than formerly supposed.—Leon Kelso.

64. The jaw muscles of some Indian birds. M. S. Dubale. 1969. *Proc. Nat. Acad. Sci. India,* **39:** 201-212.—Following an old and seldom-used terminology for the jaw muscles the author discusses anatomical variation in 13 species of nonpasserine and passerine birds. The descriptions are superficial and the illustrations are rather poor.—Joel Cracraft.

PLUMAGES AND MOLTS

(See also 14, 62)

65. Directional water-shedding properties of feathers. R. J. Kennedy. 1970. *Nature,* **227:** 736-737.—Drops of water on feathers tend to roll distally away from the skin. Barbules prevent proximal movement of water drops.—Joel Cracraft.

66. Iridescence of the "Golden Cuckoo" (*Chrysococcyx cupreus* (Shaw) through the electron microscope. (Schillerradien des Goldkuckucks (*Chrysococcyx cupreus* (Shaw)) in Elektronenmikroscop.) H. Durrer and W. Villiger. 1970. *Z. Zellforsch.,* **109** (3): 407-413. 4 figs. of 130-34,000x magnification.—The barbs and barbules of this species' feathers were scrutinized ultramicroscopically (under a scanning electron microscope) to analyze their production of iridescent hues. Melanin rods in 10-20 layers each 0.077 microns thick at intervals of 0.077 microns reflect on the "interference principle, or "colors of green leaves concept", an intense gold-green hue. Also shown was how the barbules' lamellae and pennuli "link up to form an airworthy feather vane." But the broader ornithological implications are not elaborated.—Leon Kelso.

67. Specializations of some carotenoid-bearing feathers. S. L. Olson. 1970. *Condor,* **72:** 424-430.—The author studied the barbs of carotenoid-bearing feathers in various Cotingidae, Pipridae, Eurylaimidae, Tyrannidae, Thraupidae, and Picidae. In general, the loss or reduction of barbules and the expansion and flattening of the barbs appear to be specializations associated with the presence of carotenoids in the cortical cells. These specializations are not present in all carotenoid-bearing feathers, however.—Joel Cracraft.

68. On the nature and origin of the feather colouration in the Great White Pelican, *Pelecanus onocrotalus roseus,* in Ethiopia. R. M. Baxter and E. K. Urban. 1970. *Ibis,* **112** (3): 336-339.—Supplemental superficial colors in life varied from light yellowish to deep orange-brown, deepest on breast, and were believed to be derived from ferrous oxide in the water, i.e. of exogenous origin. Yet here in the Washington, D. C. National Zoological Park, three of the same species maintain an elegant roseate tint while two or more each of the white and brown species do not, although they share the same pool whose water cannot be free of metallic compounds. The feather physiology of the particular species must be the controlling factor.—Leon Kelso.

ZOOGEOGRAPHY AND DISTRIBUTION

(See also 15, 17, 33, 37, 80, 92, 93, 94, 95, 96, 97, 98)

69. The winter quarters of British Swallows. C. J. Mead. 1970. *Bird Study,* **17** (3): 229-240.—British Barn Swallows *Hirundo rustica* winter largely in South Africa. Results of large-scale banding in Britain and in South Africa have given discrepant results about the relative proportions of birds from different breeding areas in different wintering areas. This paper shows that one reason for the discrepancy is a shift in the wintering area of the British birds. Since 1962/63 this has extended to the south and west. British birds now comprise some 34 percent of swallows in roosts in the Cape Town area, in comparison to 9 percent in the Transvaal.

The shift in wintering areas involved huge numbers of birds within a very short period of time. Mead suggests that it must have resulted either from a common environmental change or through rapid communication within the population. Perhaps a more important question is why the wintering area had been restricted before 1962. How could a British Barn Swallow, especially a young one on its first migration, have been programmed to stop at the southwestern limit of the usual range, when Barn Swallows from other populations were continuing beyond this limit?—I. C. T. Nisbet.

70. Is there a taiga in Canada? (Est li taiga v Kanade?) A. Bannikov and S. Schwartz. 1970. *Priroda,* **1.70** (9): 34-41. (In Russian.)—Not sustaining this doubt, two outstanding vertebrate zoologists, after a trip through the country, yet find many contrasts, here noted, between the mammal and bird genera of the Canadian north and subarctic Siberia.—Leon Kelso.

SYSTEMATICS AND PALEONTOLOGY

(See also 15, 85, 99)

71. *Archaeopteryx:* notice of a "new" specimen. J. H. Ostrom. 1970. *Science,* **170:** 537-538.—This paper records a further specimen found on display at the Teyler Museum, Haarlem, Netherlands. The specimen includes impressions or parts of the left manus and forearm, pelvis, both legs and feet, some gastralia, and faint wing impressions. No precise locality or stratigraphic data are known. The horny claws are preserved and Ostrom's impression is that these structures do not appear suitable for arboreal habits and he therefore *implies* a cursorial origin of avian flight! Needless to say this is a spurious basis for such an important conclusion or even suggestion.—Joel Cracraft.

72. Some ideas about the phylogenetic relationships of the Tinamiformes, based on protein characters. H. Gysels. 1970. *Acta Zool. Path. Antverpiensia,* **50:** 3-13.—Lens proteins of the Rheidae, Casuariidae, and Tinamidae were studied and compared with other avian families. The three ratite groups differ from each other in the number, concentration, and mobility of lens protein fractions. Gysels also performed immunological experiments using an antiserum against a lens component, First Important Soluable Crystallin (FISC). His results show that "the tinamou is certainly not more closely related to the fowl than to the rhea and the cassowary. In other words, tinamous belong to the Ratitae rather than to the Carinata." This conclusion is unfounded and is symptomatic of the spurious nature of the data and the systematic theory of this paper.—Joel Cracraft.

73. Variation in avian plasma proteins. J. A. Feduccia. 1970. *Condor,* **72:** 498-499.—The author examined the blood plasma proteins of a number of passerine birds. He confirmed previous studies that these proteins are of little use in the systematics of the higher categories.—Joel Cracraft.

74. The systematic relationship of *Aechmorhynchus, Prosobonia,* and *Phegornis* (Charadriiformes; Charadrii). R. L. Zusi and J. R. Jehl, Jr. 1970. *Auk,* **87:** 760-780.—The systematics of these three genera have long been in dispute, and the authors provide the most definite statement to date. On the basis of skull structure and the pattern of downy plumage, *Phegornis* can be placed in the Charadriidae. *Aechmorhynchus* and *Prosobonia,* which the authors believe are congeneric, are closely related to the Tringini of the Scolopacidae because of similarities in the structure of the skull and sternum. However, the authors believe that these genera possess sufficiently distinctive characters to be placed in their own tribe, the Prosoboniini.—Joel Cracraft.

75. The systematic position of the avian species *Metopothrix aurantiacus*. J. A. Feduccia. 1970. *J. Grad. Res. Center, South. Meth. Univ.*, **38:** 61-66. —This species has been variously included in the Pipridae and Furnariidae. According to Feduccia (p. 62) "the skull structure of *Metopothrix* definitely places it in the Furnariidae." Present evidence does not permit a definite statement regarding generic affinities within the family.—Joel Cracraft.

76. The avifauna of the Sand Draw local fauna (Aftonian) of Brown County, Nebraska. J. A. Feduccia. 1970. *Wilson Bull.*, **82:** 332-334.—This note records the presence of three additional species to the Sand Draw local fauna. The author describes a new subspecies of Burrowing Owl, *Speotyto cunicularia intermedia,* which he considers intermediate (within a temporal cline) between *S. megalopeza* Ford of the Upper Pliocene of Kansas and the Recent *S. cunicularia*. This idea is possible but another hypothesis would be that the Sand Draw fossil is a later representative of *S. megalopeza*, which is itself specifically distinct from *S. cunicularia*. Only one specimen of *S. c. intermedia* is known; unfortunately, variability within owls (especially sexual) is so great that the description of a new subspecies is questionable. Eight nonpasserines and several passerines are now known from the Sand Draw fauna.—Joel Cracraft.

77. A new shorebird from the Upper Pliocene. J. A. Feduccia. 1970. *J. Grad. Res. Center, South. Meth. Univ.*, **38:** 58-60.—This paper describes a new scolopacid species, *Tringa antiqua*, from the Saw Rock Canyon local fauna (Lower Upper Pliocene) of Kansas. The new species, based upon a nearly complete left humerus, is smaller and less robust than the Recent *T. solitaria* and is the first record of the genus prior to the Pleistocene.—Joel Cracraft.

78. Some birds of prey from the Upper Pliocene of Kansas. J. A. Feduccia and N. L. Ford. 1970. *Auk*, **87:** 795-797.—Discusses several elements of the Accipitridae, Falconidae, and Strigidae from the Rexroad and Fox Canyon local faunas of the Rexroad Formation. None of the elements could be assigned to a particular species.—Joel Cracraft.

79. A new species of Pleistocene bird of the order Accipitriformes (Accipitridae) and a new genus for the Antilles. (Nueva especies de ave Pleistocenica del orden Accipitriformes (Accipitridae) y nuevo genero para las Antillas). (In Spanish; English abstract). O. Arredondo. 1970. *Ciencias* (Ser. 4. Ciencias Biol.), Havana, No. **8,** pp. 1-19.—A new species of hawk, *Aquila borrasi*, is described for a tarsometatarsus, femur, and several phalanges from upper Pleistocene cave deposits of Cuba. The new species is considerably larger than modern species of *Aquila, Haliaeetus,* or *Buteo*.—Joel Cracraft.

80. Birds of the upper Eocene of the Paris Basin. (Oiseaux de l'éocène supérieur du bassin de Paris). (In French) J. Brunet. 1970. *Ann. Paleont.*, **56:** 1-57.—This paper represents the first revision of the upper Eocene birds of the Paris Basin. This fauna includes about 12 species of 10 families. A new species, *Ardea piveteaui*, is described for the shaft of a right ulna. This decision is unfortunate since, even when complete, the ulna is usually unreliable as a basis for discerning relationships; furthermore, the recognition of a modern genus that far back in time must be done with great care and only when the morphological evidence is compelling. *Ludiortyx hoffmanni* is transfered from the Galliformes to the Rallidae; *Palaeocircus cuvieri* is removed from the Accipitridae and assigned to the Pandionidae; *Laurillardia longirostris* is placed in the Sturnidae; and *Palaegithalus cuvieri* is allocated to the Motacillidae.

This is an important paper on an important fauna, and independent verification of some of Brunet's results will be welcome.—Joel Cracraft.

81. Endocanial casts of the birds of the upper Eocene of the Paris Basin. (Moulages endocraniens d'oiseaux de l'éocène supérieur du bassin de Paris). (In French) C. Dechaseaux. 1970. *Ann. Paleont.*, **56**: 69-72.—This note includes a discussion of the external brain morphology of *Numenius gypsorum* with brief comments on several other species. The author believes avian brains, like those of mammals, evolved principally by increasing cerebralization.—Joel Cracraft.

EVOLUTION AND GENETICS

(See also 29, 30, 81)

82. Sexual selection for size differences in two species of sandpipers. J. R. Jehl, Jr. 1970. *Evolution*, **24** (2): 311-319.—The breeding systems of sandpipers present an extremely interesting mechanism with which to investigate the selective advantage of sexual size dimorphism. While this group is perhaps traditionally thought of as one in which the females are larger (and brighter) than the males, in some species males and females are quite similar, or males are larger than females. Jehl's study investigates the tendencies for assortative mating in Stilt Sandpipers (*Micropalama himantopus*) and Least Sandpipers (*Calidris minutilla*), species in which the females are slightly larger than males and no consistent sexual plumage differences occur. At Churchill, Manitoba, where this study was carried out, both species arrive unpaired on the breeding grounds, but pairs are formed rapidly. Assortative mating appears to occur, with the small males and large females of newly-formed pairs being the first to raise young. Bill, tarsus, and wing chord measurements were used as criteria of size. Since when the first chicks hatch food sources are abundant and these sources become progressively scarcer as the marshy areas dry up, there should be a premium for raising chicks as rapidly as possible.

While small males and large females of newly-formed pairs were the first to raise young, pairs that had bred together in previous years began nesting almost immediately, regardless of their relative morphological characteristics. At this point such factors as mate fidelity and territory retention appear to exert stronger pressures than size characteristics. The basis for the original size discrimination was not determined.

Jehl then attempts to account for the advantage of sexual size differences. He feels that subdivision of the feeding area, an explanation often given as an advantage, does not suffice in this case, because there is no evidence that food is ever in short supply for adults. In this regard, it should be pointed out that his evidence for this statement is as tenuous as that usually presented to suggest that food is in limiting supply. He thus supports the above-mentioned hypothesis of the young obtaining advantages from fledging at an early date. He is clearly here going beyond his data, and it would require extensive work to determine conclusively whether one factor, year in and year out, would be more important than the other. No data are presented on fledging success of early- and late-hatched young, only of hatching success, which are equivocal. The conclusion reached above is based upon the casually observed times of drying of the marshes (no data presented) and the observation of another worker that fledging success of Dunlin (*Erolia alpina*) in Europe is greater for early-nesting pairs than late-nesting ones.

He then reviews the literature on sexual size differences in birds. He observes that many workers have concluded that the dimorphism functions importantly in alleviating intraspecific competition for food by broadening feeding opportunities for the species, but that these workers have with few exceptions failed to consider or have excluded the explanation that sexual size differences function primarily in promoting mating success. He feels that in this study the latter factor suffices entirely to explain the situation observed. With regard to his above-mentioned comments about mating success being ignored, it should be pointed out that workers in this area have often been interested in measuring the ecological consequence of the differences observed, rather than accounting for the origin of the difference. In these cases what Jehl is looking at and what the other workers have been looking at are two different things.—Douglass H. Morse.

83. Character convergence in Mexican Finches. M. L. Cody and J. H. Brown. 1970. *Evolution,* **24:** 304-310.—Following up a highly speculative paper by Cody (*Condor,* **71:** 222-239 1969), the authors made a quick trip to Mexico to see if reality conforms to prediction. It does, if one believes this hasty analysis of a complex situation.

In the one week of field work on which this study is based, Cody and Brown plotted (repeatedly in some cases) the movements of 26 unmarked territorial male finches on a four-acre study area and found that the territories of species pairs similar in plumage color (*Pipilo ocai* vs *Atlapetes brunneinucha*) and song (*P. ocai* vs *P. erythrophthalamus*) were mutually exclusive as predicted, but that territories of dissimilar species pairs (*brunneinucha* vs *erythrophthalamus* and *Atlapetes pileatus* vs the other three species) were not, again as predicted. These observations depend on the validity of the territorial maps, which at best are rough approximations due to the brevity of the study (despite the authors' contentions of accuracy within a few feet). No supporting details are given as to the number of individual observations on which the maps are based, but they could not have been many. Furthermore no assurance is given of particular conveniences in the territorial behavior of these species that validate abbreviated study, or obviate the well-recognized need for marked individuals in studies of this sort.

Song convergence in the past 21 years, which is postulated to be important in the territorial relations of *P. ocai* and *P. erythrophthalamus* is based only on a comparison of their subjective impressions with a published subjective remark by Sibley.

The casual explanations in this paper are based solely on the speculative logic in Cody's earlier paper, which postulates that convergence in such species is the result of the advantages of being evicted from the resource-poor territory of a competitive male of the other species; positive favorable habitat selection is presumed less efficient. Furthermore, Cody's hypothesis assumes that convergence in plumage color can evolve in fewer generations than does competitive exclusion, or that the limited contact of ecologically overlapping portions of the population is sufficient to cause plumage convergence in the larger, ecologically-separated portions. It seems more likely that *P. ocai* and *A. brunneinucha* converged in appearance for reasons other than those postulated by Cody, and that interspecific territoriality (if it exists) is a recent by-product of their ecological contact in disturbed areas.

Although the problem in similarity in plumage color of unrelated species is certainly an interesting one, Cody's hypothesis should have been reserved for an informal seminar until adequate study of Mexican towhees could be undertaken. One can only hope that evolutionary biologists will not cite this "textbook example" uncritically.—Frank D. Gill.

84. Evolution of diving adaptations in the stifftail ducks. R. J. Raikow. 1970. *Univ. Calif. Publ. Zool.,* **94:** 1-52.—Raikow's main aim is to trace the evolution of the anatomical changes associated with diving in the hindlimb locomotor apparatus of the stifftail ducks (Oxyurini). In doing this he has compared the hindlimb osteology and myology of three stifftail genera (which form a sequence from less to more specialized divers), *Heteronetta, Oxyura,* and *Biziura,* with a member of the primitive-like, ancestral surface feeding genus *Anas.* The paper has three main sections: (1) aquatic locomotion, (2) the tail, and (3) the hindlimb.

Although this is an extremely interesting project it can be said without equivocation that the methods employed by Raikow in his functional analysis do not represent any advance over those used by A. H. Miller in his study of the Hawaiian Goose (*Nesochen sandviciensis*) three decades ago. Moreover, these methods of study have led Raikow to what are probably very erroneous conclusions about the functions of numerous muscles.

First, his visual observations of only *Anas* and *Oxyura* are an inadequate basis for making certain statements in the section on aquatic locomotion. For example, surely Raikow did not actually observe *abduction* of the femur (in contrast to protraction or retraction) so that the bone was in or near the horizonal plane (p. 2). This extreme movement is totally impossible in all birds due to joint restrictions and lack of the necessary musculature. Likewise, I know of no empirical evidence to suggest that the femur and tibiotarsus remain "still" in slow swimming (p. 4). I strongly suspect that if swimming birds were analyzed using high-speed cinematography or electrical methods, these two elements would follow a predictable pattern of movement even in slow-swimming birds. In any case, Raikow's descriptions of locomotor behavior are too superficial to provide a basis for anything but the most general correlations with structural and functional differences.

The functional conclusions of the last two sections dealing with the tail and hindlimb are based upon several premises that neglect the most elementary principles of muscle physiology. Raikow uses the dry weights of the muscles expressed as a percentage of total muscle weight as a comparative index for the amount of force generated (he uses the word "strength" which is very ambiguous). This is simply false unless he can demonstrate (which he cannot) that the muscles have the same fiber architecture including the same length of fibers, angle of pinnation, arrangement of aponeuroses (i.e., any parameters which will influence the numbers of fibers and not the mass of the muscle). He fails to realize that muscle mass does not necessarily have a direct relationship with the amount of force that a muscle can produce.

Raikow also places great faith in his measure of the mechanical advantage (force arm/ resistance arm), of each muscle. He assumes that muscles which attach closer to the fulcrum of an element move that element through a greater arc than those muscles attached further from the fulcrum and that a large mechanical advantage indicates adaptation for strength and not speed (he assumes *vice versa* for a muscle or small mechanical advantage). Once again these premises are false unless one disposes of a whole list of assumptions which include the muscles being compared having (a) the same number of fibers, (b) the same angle of pinnation, (c) the same length of fibers, (d) the same physiological types of fibers (twitch or tonus) in the same proportions, and so forth. Any variable that might affect the patterns of the length-tension curve must be discussed, as must the influence of body size on the mass of the element being moved relative to muscle force. It is for these reasons that Raikow's use of mechanical advantage lacks *reliable* explanatory value.

With the use of an analysis of muscle architecture and some considerations of muscle physiology it would have been possible to state a great deal more about the functional significance of some of the muscles. More plausible functional explanations can be offered for some of the structural differences Raikow observed. Also, if he had considered the importance (greatly underestimated by most functional morphologists) of posture in the hindlimb locomotor apparatus (he neglects this almost entirely), Raikow could have provided a much better analysis. Thus on p. 40 it is more plausible to discuss the increase in size (given the previously mentioned assumptions) of the abductor muscles in the more terrestrial forms in terms of posture rather than movement of the femur. The femoritibialis almost certainly plays no role in actively providing force against the ground in walking (p. 41), but rather extends the knee joint (against little resistance) as the leg is brought forward for the next step. As in most terrestrial birds the knee joint of ducks is probably flexing slightly as the intertarsal joint is being extended during the power stroke. As I pointed out in an earlier review (1971. *Bird-Banding* **41:** 254) the insertion of the iliotrochantericus posterior is probably dorsal to the axis of the femoral head and thus acts as a retractor rather than a protractor. This is true for the tree duck *Dendrocygna bicolor* and is probably true for the ducks dissected by Raikow who listed the insertion as being (p. 34) "at about the level of the acetabulum." This muscle probably is most important in resisting protraction of the femur during posture rather than in retracting the femur. This would explain its greater size in the more terrestrial species.

Several other points which could have improved his analysis include a consideration of joint structure and function and reliance upon fresh material rather than preserved material for his functional conclusions. Some very interesting differences in joint and ligament organization could probably be correlated with the different modes of locomotion.

Readers of this paper should be aware of the statement on p. 48 that the abductors are absent in *Heteronetta* when they are said to be present on p. 33 and figured on p. 27.

The most important contribution of this paper is the discussion on pp. 48-50 in which Raikow attempts to treat the morphological differences of these ducks in terms of primitive-derived sequences. In doing so he is following the most meaningful approach to the use of muscle differences in avian systematics. Those workers interested in applying myological data to avian systematics would do well to read these pages.—Joel Cracraft.

85. Isozyme polymorphism in the Ruff (**Aves,** ***Philomachus pugnax***) **a species with polymorphic plumage.** A. Serge, R. C. Richmond, and R. H. Wiley. 1970. *Comp. Biochem. Physiol.*, **36:** 589-595.—Twenty-two captive Ruffs were examined for polymorphisms in blood serum proteins. Extensive polymorphism was found in five enzymes. Some possible explanations for this variation are discussed but no definite answers are reached.—Joel Cracraft.

FOOD AND FEEDING

(**See 21, 26, 27, 29, 48, 59**)

SONG AND VOCALIZATIONS

(**See also 83, 91**)

86. Evolution of design features in coding of species specificity. M. Konishi. 1970. *Amer. zoologist,* **10:** 67-72.—This surprisingly is a fresh approach to the role of voice in avian evolution.—Leon Kelso.

BOOKS AND MONOGRAPHS

87. ***The Capture and Banding of Birds. Part III:*** **Capture with bow-nets, hand-nets, by hand, capture in the evening and at night, capture at the nest and watering-places.** ***Part IV:*** **Capture with clap-nets, rocket- and cannon-nets, principles of scientific bird-banding, bird-banding organizations, and techniques of age- and sex-determination.** (Vogelfang and Vogelberingung. Teil III: Fang mit Schalgnetzen, Kätscher und Hand, Abend-und Nachtfang, Fang an Nest und Tranke. 116 pp. 68 illus. 7.80 M. Teil IV: Fang mit Vogelherden, Zugnetzen, Raketenund Kanonennetzen. Grundsatzliches zur wissenschaftlichen Vogelberingung, Planberingung und zur Alters- und Geschlechtsforschung. 207 pp. 109 illus. 15.20 M.) Hans Bub 1968, 1969. A. Ziemsen Verlag, Wittenberg-Lutherstadt.—This appearance of these two volumes completes what is certainly the most comprehensive work available on trapping techniques and banding methodology. (For a review of parts I and II, see *Bird-banding,* **39:** 241). Trapping techniques, from the most ancient to the most recent, are presented in meticulous detail. As an example, there are 82 pp. and 63 illus. on a variety of clap-nets and appurtenances including 14th Century drawings and 20th Century photographs. The books gave me an opportunity to revisit, in text and pictures, the banding station at Cedar Grove where I spent many happy days. I was amazed at the fidelity and detail in the account of the trapping techniques I knew so well. In addition to trapping, banding, and other marking techniques the last volume presents information on banding organizations, banding schemes, systematic efforts to band certain species, ageing and sexing methods, data recording techniques, and some aims and purposes of banding programs. The books should be available from any large bookshop dealing in foreign books. The books are part of the Neue Brehm Bucheri, and are published in East Germany. Some avid bird-bander with a good command of German would do the English-speaking world a considerable service by translating these volumes, an endeavor that I am sure could be arranged with Herr Bub.—Helmut C. Mueller.

88. ***Home is the Desert.*** Ann Woodin. 1970. Macmillan, N. Y. 247 pp. $2.95.—This is a new paperback edition of the popular 1964 book by the wife of the director of the Arizona-Sonora Desert Museum near Tucson. It is from the

Museum that come many of the baby animals reared in the household, although this fact does not adequately explain the dedication: "To my husband, without whom I would have had nothing to say — almost."

The cover invites comparison with *Born Free,* but the resemblences are superficial. Ann Woodin is clearly more literate than Joy Adamson, but Woodin's text tells little about herself and those close to her while Adamson's books reveal, apparently naively, a great deal about personal relationships. And Mrs. Woodin is not nearly as keen an observer of animals and their behavior.

The life of the desert and a family of four boys (plus innumerable animals of every sort imaginable) comes through, however, often in delightful ways. I think this book improves as one goes along, so don't abandon it in the early pages. Woodin seemed to have slight trouble "getting into gear," some early passages being vague, descriptions of beauty labored, integration of fine literature strained, and metaphores puzzling, if not mixed. But later the book becomes lively and natural. A. A. Milne is cited with much greater authority than Henry David Thoreau, and descriptions of nature become enchanting.

Many beasts were raised in the Woodin home: bobcats, a wolf, a badger, uncounted snakes, spiders — virtually anything that could be caught in the desert or brought into the Museum to be cared for. Snakes are treated quite fairly, and Legs the tarantula is actually given sympathy. Jimmy the raven is perhaps the most interesting household pet, his antics being sufficiently described to deter me from ever having so bright a bird species in my own home. In general, though, there are simply too many animals. While one gets to know something of bobcats and the wolf, most of the animals merely fleet by in some sort of parade. The photographs are generally magnificent, featuring primarily the boys and the succession of bobcats.

Stories of this genre have been scarce since *Born Free,* perhaps because it is so tough an act to follow. Ann Woodin's *Home is the Desert* is one of the best of the current crop.—Jack P. Hailman.

89. ***The Random House Book of Birds.*** Elizabeth S. Austin and Oliver L. Austin, Jr.; illustrated by Richard E. Amundsen. 1970. Random House, New York. viii + 131 pages. $4.95.—Bennett Cerf and his boys did this one up nicely. The wife-and-husband team who wrote the text are well qualified, and in some ways the book is a midget version of Oliver Austin's *Birds of the World.* The colored paintings by Amundsen are good throughout, although the artist clearly does better with some kinds of birds than others (*e.g.,* the thrushes on p. 100 are rather insensitive).

The book is subtitled "A family encyclopedia of birds around the world," but is not exactly that, since the emphasis and examples have American species as their focus. I was not sure what kind of "family" the subtitle referred to, so turned to the introduction, which was no help whatever—nor is there a table of contents. (By the by, the first page of the introduction is viii and the next one is 1, which gives the book a certain uniqueness.) The dust jacket says the book will "delight every member of the family" and that it gives general information "on 96 family groups of birds". Perhaps the *double-entendre* was intentional.

At any rate, the "family groups" are not always families, and they are listed in alphabetical order rather than the usual sequence. This latter seems sensible at first, since it obviates the need for an index. However, if you look up "Kiwi" the text says "See Ostriches and Other Ratite Birds," so you might as well begin with the index. A few entries are not avian groups: Domestic Birds, and Voice. But don't try "Nest" because it isn't even in the index.

Entries are usually informative and factual, but occasionally one finds statements such as "The male parent [American Robin] feeds the chicks and shows them how to pull worms out of the ground . . .". Plugs for conservation and ecology are excellent: "Gulls are scavengers and tidy up after litterbugs." However, the site of the plaque to the last wild Passenger Pigeon is given as "Wyalsing" State Park to which we Wisconsiners must object (it is Wyalusing). And it really is not much help to learn that most parrots lay from "1 to 12 round white eggs."

Writing a popular book is a very difficult job, and this is a good one as they go. I think, though, that it fails to measure up to the previous standards that the prolific Austins have set for themselves.—Jack P. Hailman.

90. ***Communities and Ecosystems.*** R. H. Whittaker. 1970. (Current Concepts in Biology Series.) The Macmillan Co., New York, London. XI - 162 pp. 36 figs., 16 plates, 6 tables. $3.95.—This university-text designed book constitutes a summary of the advanced concepts of a theoretical ecologist whose dis cussions have won much favor here and abroad. It having been reviewed in detail elsewhere, we would only note that the discussions are very ornithologically oriented for a general text, citing many bird references, and that many a reader might find how far his understanding lags behind recent advances. Among these, to so put it, plants, particularly desert shrubs dominant over wide areas, enforce territorial dominance as definite as that of birds; i.e. by vaporous and liquid effluvia they repel lesser herbaceous species from their vicinity, and also confine moisture under the soil.—Leon Kelso.

91. ***Bird Vocalizations: Their Relation to Current Problems in Biology and Psychology.*** Edited by Robert A. Hinde. 1969. Cambridge University Press, N. Y. xiii + 394 pp. $13.50.—Sub-subtitled "Essays presented to W. H. Thorpe", this volume is a collection of review-like papers principally by workers who were students, associates or otherwise strongly influenced by Thorpe. Most of the essays are either excellent reviews of the literature on some topic, or else summary papers tying together a long string of studies by the author himself. Konishi and Nottebohm do a superb job of showing how the old instinctive-learned dichotomy no longer leads to interesting questions about the complex interactive development of bird song. Stevenson recounts her experiments showing that hearing one's own species' song is reinforcing much like classical psychological rewards such as food. Immelmann reviews his work on song development in estrildid finches, and there are three papers on physiological aspects of song (brain stimulation studies, hormonal studies, etc.). A great deal of the work is devoted to studies of the functions of vocalizations, including territory, courtship, synchronization of hatching, social behavior, etc. The papers are all by persons who have a solid first-hand knowledge of their areas: Jerram Brown, Andrew, Brockway, Mulligan, Hooker, Falls, Vince and Crook. Lanyon discusses the use of vocalizations in avian taxonomy, and Thielcke reviews the literature and his own studies on the geographic variation in vocalizations.

Only three of the papers could reasonably be called original contributions, in the sense of dealing with topics possibly not familiar to those who follow the main theme of bird song literature. Peter Marler presents a fine explanation of vibrato in a paper entitled "Tonal quality of bird songs." When a single tone is produced, and then modulated, one perceives it either as a sort of alternation between other tones or as a complex sound of uncertain pitch. The perceptive effect depends in part on the basic frequency, the rate of modulation and the range of modulation. Marler explores with great clarity the relationships, and demonstrates how the band-pass filter characteristics of the sonograph machine create similar effects on printed sonograms. The major point is that vibrato, or tonal structure, of vocalizations allows a great diversity of signals, and should be given more attention by those attempting to unravel auditory-communication in birds.

Edward Armstrong has penned a most fascinating essay on the influence of bird song upon literature of widely different cultures, especially emphasizing how man's appreciation of bird song has altered through history. Finally, Joan Hall-Craggs looks at the development of music and its parallels with bird song in an essay entitled "The aesthetic content of bird song." Robert Hinde is to be congratulated for managing in this volume to keep science and aesthetics between the same covers; and, more widely, for having produced a solid contribution to the study of birds and behavior. Hall-Craggs quotes Percy Scholes: "There are two musical races in the world—the birds and the humans." Phylogeny is not the only tie that binds us to the coinhabitants of our terrestrial spaceship.—Jack P. Hailman.

92. ***A Field Guide to Australian Birds: Non-Passerines.*** Peter Slater. 1971. Livingston Publishing Company, Wynnewood, Pa. xxxii + 428 pp. $10.—Here is another fine new field guide. The recent plethora of field guides reviewed here suggests a few general comments on reviewing them. If reviews of ornithological books are helpful at all, they may be *least* helpful when they concern field guides. With field guides the proof is in the pudding: only varied experience in the field really tells the worth. Since the reviewer rarely gets the opportunity to use the guide immediately, especially when it covers some distant region, he can only comment on the mechanical aspects of the guide: its arrangement, quality of illustrations, accuracy of text, and so on. By such measures, the present guide is a real winner.

There is to be a companion volume on the passerines of Australia (projected publication data unknown). Livingston, known to many of us not as a publisher of books but of checklists, is to be congratulated for undertaking a book for down under. Although Slater is the only name on the cover, in fact the maps were done by Eric Lindgren and various sections of text written by familiar names in Australian natural history: J. Calaby, G. Chapman, J. Forshaw, H. J. Frith, F. T. H. Smith and G. F. van Tets.

The first 131 pages are plates opposite a listing of key identification features. The plates, by Slater, are all more than adequate, and some are fine, indeed. There are no arrows to key features, but the owner can pencil these in for rapid identification. The species accounts follow the plates, and contain the usual information: common and Latin names, general description (called "looks"), voice, habitat and range (world wide). Nearby, and usually at the bottom of the page, is a range map of the species in Australia. There is a checklist in the front, along with the usual front matter. This volume treats a huge 394 species, and the next will have about 310 more.

My one complaint has to do with the binding, and it is serious. Although the pages are firmly sewn, the back is of hardboard similar to the cover materials. On my first opening of the book, the firm back caused the book to tear the endcover from the back hard cover. Put simply, there is no way to open this book without tearing it, and it is impossible to open the book flat under any conditions. I suspect that most users will tear the covers off after a very brief time in the field, since they are going to come off anyway. I have never before seen a book bound in this way, and with some luck maybe I never will again.—Jack P. Hailman.

93. ***Common Australian Birds*** **(2nd ed.)** Allan and Shirley Bell. 1969. Oxford Univ. Press, London and elsewhere. xi + 218 pp. $9.50.—This is a sort of beginner's field guide, illustrating a hundred of Australia's roughly 650 species. The psychologist and historian of science Julian Jaynes remarked recently on the unfortunate habit of updating books in new editions. The 15-year old first edition of this well known volume was fine for its day; the revision is not up to present-day standards for field guides. The illustrations are merely adequate, if that. Although all birds appear in color, they often seem as caricatures of the real thing: the beaks are mis-shapen (and frequently too small), while the eyes are lovingly but inaccurately large. Only males in breeding plumage are shown. Latin names are not always up to date, and unfortunately common names are too numerous—which is to say that a species heading may read "Black or Grey Duck", "Mountain Duck or Chestnut-breasted Shelduck," etc. A great service performed by Peterson's guides in America was to standardize common names, not encourage their proliferation.

Each bird illustrated on the right-hand page is described on the left, where notes on its behavior and habits are included. The notes are chatty, and of possible attractiveness to a beginner, but tend to remind one of a "padded" freshman theme. There is, no doubt, some place for this kind of book—at, perhaps, about a third the price.—Jack P. Hailman.

94. ***A Guide to The Birds of South America.*** Rodolphe Meyer de Schauensee. 1970. Published for the Academy of Natural Sciences of Philadelphia by Livingston Publishing Co., Wynnewood, Pa. xvi + 470 pp. $20.—Here it is! The massive, authoritative culmination of de Schauensee's contributions to South American ornithology, this ambitious attempt to describe the nearly 3000 species making up the world's richest continental avifauna is sure to spark much new interest. It is now possible for every ornithologically minded visitor to tote along with him to South America a virtual library of field guides between two covers, and with great certainty in most cases to identify everything he sees. The plates by John R. Quinn and Earl L. Poole, and the line drawings by George M Sutton, illustrate more than 600 genera (or about three-quarters of the South.

American genera). The illustrations are useful, and while aiding in identification, cannot assure it.

If one knows the family of the bird in question, or can determine the family by the plates, he then turns to the family account via the table of contents. The account begins with a short description of the family, then proceeds to a key-like "aid to identification" section. Here one scans the major categories until finding the one that describes his bird. For instance, if it is a hummingbird there are ten categories, and your bird might fall into "underparts all or mostly uniform buff to chestnut." Within this major category are minor ones, such as "bill distinctly curved" and "bill straight or nearly so." This minor category lists species, but may be broken down further: e.g., "(basal portion of mandible pale)" or "(base of mandible dark)". If your bird happened to be the last mentioned, you are in luck, because it could be only one of the 233 hummingbirds. However, if the basal portion of the mandible is pale, then you find a list of numbers (14 in this example). The numbers refer to the species accounts within the family. Here more detailed descriptions, plus known ranges and habitats allow identification partly by process of elimination. The system may not always work, of course, but due care has been taken to multiple-list species in different categories where variation, sex differences or age differences in plumage require. And I for one appreciate the departure from cumbersome dichotomous keys.

Naturally, the true worth of a field guide is measured by how well it serves one in the field, and I would be delighted to field-test this one if someone will foot the bill. For greater details of ranges, one should utilize de Schauensee's 1966 *Species of Birds of South America,* which has been summarized (and updated) in this work. Surely this work will bring about numerous reports of extra-limital birds, promoting a solid ornitho-geography of the continent. But it should also stimulate much work on life history, behavior and ecology now that one no longer has to have a museum nearby for authoritative identifications. This invaluable volume should be on the shelf of everyone interested in birds.—Jack P. Hailman.

95. ***Roberts Birds of South Africa,*** **3rd edition.** G. R. McLachlan and R. Liversidge. 1970. John Voelcker Bird Book Fund, Cape Town, South Africa. xxxii + 643 pp. R6.75.—Here is an old friend revised for a second time by McLachlan and Liversidge. The book treats 875 species with the usual text headings of identification features, distribution, habits, food, voice and breeding data. Beside each account is a very helpful map of South Africa, the rear end-paper giving an enlarged map with place names and political divisions, while the front end-paper map provides the main ecogeographical regions. Some of the newer plates by J. Perry and K. Hooper are black and white field illustrations with arrows to key features, in the modern field guide style. However, most of the illustrations are the old color plates of Norman C. K. Lighton—in some cases beautiful, but certainly less useful for quick identification. In the preface to this edition, the Trustees of the Voelcker Fund promise an attempt to replace these plates in subsequent editions if this becomes financially feasible.

Since 1940, when Roberts published the original edition, this book has been the standby for field identification and general information in South African ornithology. The authors and Trustees of the Voelcker Fund are to be congratulated for continuing to update this valuable volume. Not only is the information current, but the volume has been reset in more modern and pleasing format. It is a large volume to carry in the field, but I should not want to be without it.—Jack P. Hailman.

96. ***The Birds of Puerto Rico.*** (*Las Aves de Puerto Rico.*) Virgilio Biaggi. 1970. Editorial Universitaria, Universidad de Puerto Rico, San Juan. 371 pp. $6.50. (In Spanish).—The promotional material claims that Professor Biaggi's book supersedes Bond's out-of-print *Field Guide to the Birds of the West Indies,* but it does no such thing, even for Puerto Rico. (By the by, a new edition of Bond's classic is tentatively scheduled for "early 1971".) Although no field guide, Biaggi's volume is an attempt at a complete book on Puerto Rican birds, 239 species being treated.

The species accounts begin with the Latin name, complete with etymology (which is fun). There follow descriptions of the adults, young, nest and eggs, habitats, distribution, etc. The text is in simple Spanish, to the delight of the

American reader. The color plates by Lucila Madruga de Piferrer abound, but Christine Boyce's line drawings are better. At any rate, all the illustrations are useful in identification if you lack Bond's *Field Guide*..

There are special chapters making up the front matter. Following the Introduction there is a section on the history of Puerto Rican ornithology. Then comes a section on animal classification, which seems to me of tangential interest. A section on avian migration follows, and then one on the economic importance of birds. Hopefully this front matter will generate interest in and conservation of Puerto Rican birds.

Considering today's prices, this volume is a real bargain.—Jack P. Hailman.

97. Check-list of the Birds of New Mexico. John P. Hubbard. 1970. *New Mexico Ornithol. Soc. Publ.*, No. **3,** 103 pp. $2.50.—This is an attractive, complete and informative state checklist that should accompany anyone traveling through New Mexico. It is apparently a photo-offset printing, typed in several typefaces of the IBM selectric typewriter; the method is extremely successful, allowing not only ordinary face and italics, but also script—all used for different purposes.

The list contains a mammoth 476 species: 401 collected, 12 photographed only. Another 51 species are admitted on the basis of sight records, seven more were introduced at one time or another, three are based on questionable specimens, and two "are recorded in the literature on the basis of now-withdrawn sight records" (whatever that means). Hypothetical species are placed sequentially in the list, but enclosed in square brackets.

Front matter briefly discusses the setup of species accounts, the history of the state's ornithology, habitats in the state, sources of data and other useful tidbits. Species accounts follow the A. O. U. checklist order, are grouped by families, and contain: common and Latin names, status and habitat at different seasons, and sources of data. My one criticism is of the arbitrary symbolism used: asterisk indicates breeding, the degree-sign probable breeding, double-quotes specimen, single-quote photograph, and so on. The user will have to spend some time becoming familiar with this notation.

The endpaper maps by Mary Huey are useful for those of us who do not happen to know where De Baca County or Jemez Springs are located. Hubbard's volume sets high standards for state lists to follow.—Jack P. Hailman.

98. ***A Guide to the Birds of Taiwan.*** Sheldon R. Severinghaus, Kuo-wei Kang and Paul S. Alexander. 1970. China Post, Taipei. xix + 130 pp, line drawings and 35 color photos. (In English and Chinese.)—Nearly 400 species of birds have been recorded on Taiwan, which is an island having roughly half the land area of Maine or of Indiana. This book introduces 117 of the commonest species, each with a competent sketch, and a text on field marks and habits in both English and Chinese. The photographs are of hand-held birds caught at a banding station. This is a useful and attractive pocket-sized field guide that the traveler to Taiwan, and perhaps the living room voyager, will welcome.—Jack P. Hailman.

SPECIAL BOOK REVIEW

99. A comparative study of the egg-white proteins of passerine birds. C. G. Sibley. 1970. *Bull. Peabody Mus. Nat. Hist., Yale Univ.*, No. **32:** 1-131.—Generally speaking the higher classification of birds has remained relatively stable for the last 50 years. One of several reasons for this has been the paucity of original comparative analysis—whether morphological or behavioral. Another reason has been the failure of ornithologists to develop and apply new techniques that provide additional data which are taxonomically useful. For about 15 years Professor Charles G. Sibley and his coworkers have pioneered one of the few novel approaches to avian classification—the use of "molecular systematics". Most of their research has utilized data gathered from studies of egg-white proteins, and the early results were presented in Sibley's review paper published in 1960 (*Ibis*, **102:** 215-284). Although there have been other publications on specific groups of birds (sometimes using proteins other than egg-whites), the present paper on passerines represents a summary of the last ten years' work; a separate *Yale Pea-*

body Bulletin discussing nonpasserine groups will be published in the near future. Despite the impressive amount of data presented by Sibley *et al.*, no one has attempted a critical analysis of their methods nor has anyone examined the substance of their results. Professor Sibley's monograph on the passerines provides an excellent opportunity to judge the impact of molecular systematics on avian classification and phylogeny.

The paper is divided into four main sections. First, a discussion of the techniques and methods of interpretation of egg-white protein patterns as produced by starch-gel electrophoresis is presented. This is followed by two sections—one covering non-oscine passerines, the other oscines—in which the protein patterns of each family are evaluated and compared. Finally, there is a brief section which attempts a synthesis of the wealth of factual information gathered in this study of some 668 species.

My comments on Sibley's monograph can be divided into three general topics: (1) systematic theory, (2) techniques of gathering electrophoretic data, and (3) interpreting the electrophoretic data.

(1) *Systematic theory*—Sibley states his purpose early in the paper (p. 7): "I will attempt to review the pertinent literature on passerine classification and to present from a study of the egg-white proteins some new data bearing upon the problems of family level relationships". Most of us concerned with the higher level systematics of birds would agree that the eventual classification should be an accurate reflection of the relationships of the various taxa. Thus the primary goal of any study that gathers taxonomic information should be the elucidation of the relationships of the organisms; ideally a classification should be constructed only after the problem of relationship is solved. (Regardless of how one wants to define "relationship," i.e., whether only in terms of the recency of common ancestry or whether one also wants to include some measure of morphological divergence, the first basic step must be to determine which taxa had a most recent common ancestry; only then can morphological divergence be considered). The central question, then, is *how* does one determine the relationships of avian families? The answer to this question must first, and foremost, have a strong theoretical foundation, and it is on the basis of theory that any comparative study will or will not yield meaningful conclusions about relationships. Dr. Sibley's theoretical approach to the use of egg-white proteins as comparative data, while not differing from many of the current methods of morphological analysis, is open to some serious questions, which in turn affect the validity of his taxonomic conclusions.

In essence, phylogentic affinity can be based solely on a recognition of shared character-states that were inherited (i.e., derived) from the common ancestor. In any study a worker needs to provide a character analysis in which an attempt is made to discern primitive and derived character-states. It is not only the homology of the characters that is in question but also the homology of the character-states that must be examined. It follows that if primitive-derived sequences cannot be recognized, then there is little justification to expect that assertions about affinities will be reliable. To my knowledge no one has attempted a character analysis of egg-white protein patterns. Sibley and his followers have relied on "similarities" of the patterns to indicate relationships without considering whether it is primitive or derived similarities they are comparing. If primitive similarities, then they offer no evidence for relationship. Thus, it is quite one thing to state (p. 20) "because there do exist consistent similarities within groups of related species [determined to be 'related' on the basis of previous evidence], and consistent differences between the members of different groups, we are justified in suspecting that such clusters may be natural assemblages" and quite another to claim that a character analysis of egg-white proteins has produced these assemblages. The above is brought into focus by a recent paper of L. H. Throckmorton ("Biochemistry and taxonomy", 1968, *Ann. Rev. Entomology,* **13:** 99-114) in which he states (p. 109) "the objective of the phylogeneticist is to discover sets of characters with specific states that identify certain species as derivatives from certain internodes. The characters of the phylogeneticists identify internodes on a dendrogram. They are not estimates of similarity."

At first glance Sibley seems to qualify the claim that egg-white proteins are useful in determining relationships with the statement (p. 39) that the "patterns are useful in suggesting lack of relationship and in demonstrating the cohesion of a closely related group but they cannot, alone, provide a firm basis for suggesting an alliance between groups for which there is no other evidence of relationship". This is a revealing confession and is symptomatic of the problems we have had with passerine classification. While the patterns might suggest lack of relationship, we might reasonably question, of course, how one can *demonstrate* "the cohesion of a closely related group" unless one can show that a derived condition of the character being used actually helps define that group. What we desperately need are taxonomic characters which, by themselves, can be used to define monophyletic assemblages *independent* of previous opinions. At its worst, reliance on prior taxonomic decisions for the interpretation of one's data merely compounds and re-enforces errors of judgment that may already exist. Thus, even though "some patterns of the cardueline finches are virtually identical to that of Zosterops" (p. 85), Sibley pursues this similarity no farther, presumably because a *Zosterops*-cardueline relationship would conflict too drastically with previous ideas about the relationships of these groups. To carry this reasoning to the important issue, Sibley suggests a relationship between many groups with similar patterns when a strong argument can be made that there is little concrete evidence to suggest a relationship in the first place (most of the evidence is strictly subjective opinion that is notable for its lack of analysis of hard data). Examples of this are the supposed relationships between the Dicruridae and Pycnonotidae (p. 50), Laniidae and Corvidae (p. 53), Cracticidae and Paradiseidae (p. 55), the tribes of the Timaliidae (p. 63), and so forth. How do we objectively determine whether similar protein patterns indicate relationship or not?

(2) *Techniques of gathering electrophoretic data*—Anyone slightly familiar with electrophoresis will know that a huge literature exists about this technique of biochemical analysis. Although there are many kinds of electrophoresis, Sibley has chosen the starch-gel method which most workers consider very effective in providing excellent separation of the different components of a protein mixture. I am not trained to discuss the details of starch-gel electrophoresis, but my impression is that Sibley is very careful in his technique and that he attempts to standardize the procedures as much as possible. Since variation in the electrophoretic patterns frequently involves rather minor changes in mobility, the importance of using precisely uniform techniques cannot be overemphasized. Thus, it probably would have been valuable if Sibley had discussed the technical problems in more detail, pointing out those aspects that introduce error into the patterns. Except for the obvious topics of denaturation, sample concentration, and "artifacts", no other technical problems are mentioned. Because "the relative values of the starch concentration and the ionic strength are the fundamental determinants of the detailed patterns observed" (Smithies, 1955. *Biochem. J.*, **61**: 640), one might have expected Sibley to say something about this aspect. Kunkel and Trautman (1959. In *Electrophoresis theory, methods and applications*, Academic Press) also claim (p. 236) that "small variations in the type of starch and the time and conditions of hydrolysis lead to differences in resolution". One assumes that Sibley went to efforts to standardize the starch solution, but he probably should have emphasized this and given details.

(3) *Interpreting the electrophoretic data*—Sibley has a short section on the interpretation of electrophoretic patterns but unfortunately certain parts are confusing and make it extremely difficult to follow some of his arguments in the systematic section. For example, in his figure 2 he has labeled the components of only two of the four electrophoretic patterns. Most patterns can be understood when reference is made to the text but here terms are tossed about with abandon: ovoconalbumin (=ovotransferrins), conalbumins, component 18 (=ovomacroglobulin), ovomucoid, ovalbumin, pre-albumin, etc. Because Sibley does not carefully label these introductory figures and those later in the paper, there are numerous instances when it is difficult to determine whether one is comparing the same protein or not. Furthermore, Sibley often mentions the presence of a fast or slow "component" but does not tell us what he thinks this component is (e.g., p. 33). This is a serious omission and is extremely important if one is trying to compare patterns.

Another important point not explained by Sibley was why the patterns of numerous species which he examined were not included in the published figures. For example, only 6 of 10 species of the Timaliidae, 6 of 16 species of the Pycnonotidae, 29 species of 46 in the Turdidae, and 13 of 26 species of the Muscicapidae were illustrated. Many of the omitted patterns represented diverse genera within the family. Of the 29 species of Turdidae illustrated 15 were from the genus *Turdus*. It would have been helpful to know why particular patterns were not illustrated, especially since many of the families appeared to show considerable intergeneric variation in mobilities.

The interpretation as to whether protein patterns are "similar" or not is bound to cause problems, but in Sibley's case it appears to depend in many instances on the way current taxonomic winds are blowing rather than on the data presented by the proteins themselves (see also above). A few examples will suffice.

On p. 37 Sibley claims that the starch-gel patterns of *Platypsaris, Chiroxiphia,* and *Phytotoma* are similar to each other and to the Tyrannidae and Furnariidae. However, the patterns of these three genera as shown in figure 4 do not support this conclusion. *Platypsaris* and *Chiroxiphia* show some similarities but the mobilities of their proteins are not at all similar to those of *Phytotoma.*

"The egg-white patterns also indicate that *Gymnorhina* and *Strepera* are closely related" (p. 54). In figure 10 it is not apparent to me that the patterns are "similar".

"Although recognizing that *Bubalornis* is a distinctive genus, not extremely close to the ploceines, I believe it is probably closer to them than to any other group" (p. 93). Surely this cannot be based on egg-white evidence since the patterns of *Passer*, for example, appear closer to *Bubalornis* than do those of the Ploceinae.

A major shortcoming of the systematic section is Sibley's failure to discuss adequately the variation seen within some families and to relate the breadth of this variation to his comparisons between families. Frequently he claims that the species of a genus or family show a common pattern when this is not readily apparent in his figures. "The Type A starch gel electrophoretic patterns of the egg-white proteins of the various species of larks are essentially identical" (p. 49). The egg-white patterns of the seven species of larks shown in figure 7 do not appear to me to be "essentially identical" as they show considerable variation in the mobilities of several components. This variation is as great as between some families. Although it may be my inexperience in interpreting these patterns, the burden falls to Sibley to explain how he has interpreted these patterns to be similar, especially when the figures do not unequivocally support his conclusions (see his discussion on p. 12).

It becomes increasingly apparent as one reads the systematic section that, despite his disclaimers to the contrary (e.g., pp. 17, 110), electrophoretic coincidences (i.e., two proteins with different amino acid sequences having the same electrophoretic mobility) may be quite common. Sibley believes that coincidence will be important only with proteins of unknown homologies or with patterns that are "simple". First, Sibley himself states (p. 15) that unlike the situation in some nonpasserines "the homologs of ovomucoid, ovalbumin and pre-albumin [few egg-white proteins are now left] have not been definitely identified in passerine egg white although it is reasonable to assume that they are present". But the homology of the different components within the passerines also becomes an almost impossible problem and this fact is repeatedly demonstrated throughout the family accounts. This not only takes the form of failing to identify or discuss certain ambiguous patterns or components but also sometimes an outright admission of confusion. Thus (p. 71), "It is clear from an examination of patterns produced by samples of different concentrations that the 'ovomucoid' band in the *Prinia* patterns *has the same mobility* as the 'ovalbumin' band in the sylviids and that the fastest ('ovalbumin') fraction in *Prinia has the same mobility* as the 'pre-albumin' of the sylviids. Thus the 'ovalbumin' of the sylviids may actually be homologous to the 'ovomucoid' of turdids and the 'pre-albumin' of sylviids may be the homolog of the 'ovalbumin' of the turdids" (emphasis mine). And yet he believes it probable that "the Sylviidae, Muscicapidae and *Prunella* are more closely related to one another than any one of them is to the Turdidae" (p. 115). The problem is simple—if you cannot recognize homologous characters you have no objective basis on which to form taxonomic decisions. With the distinct possibility of uncertain homology in the patterns of many families, what are we to believe about the probability of coincidence?

Second, Sibley nowhere defines what he means by "simple" patterns, and only when there is some doubt in the literature about relationships does he appear to invoke coincidence: (p. 78) "the patterns of both groups [Laniidae and Corvidae] tend to be unusually simple and thus the probablity of coincidental similarity is high". But many other groups (e.g., *Zeledonia,* Thraupidae and some other nine-primaried passerines, Passeridae, Zosteropidae, etc.) have "simple" patterns. Are these also subject to a high probability of electrophoretic coincidence? In his discussion of taxonomic significance of electrophoretic data (p. 17) Sibley did not discuss the effect of pattern simplicity on the probability of coincidence. An important area for future research is the significance of electrophoretic coincidence including the differences in mobility shown by homologous proteins that differ in a variable number of amino acid substitutions.

Sibley obviously believes egg-white proteins tell him something about the "degree of genetic relatedness" (pp. 21-22, 109) of different taxa but like R. Sokal (1969. *Systematic Biology,* p. 390) I believe such a concept is ambiguous and should be used only when it has some useful purpose. To believe egg-white protein patterns tell us anything about the "degree of genetic relatedness" of avian families (let alone the genomes of species!) is nonsense. First, electrophoretic patterns tell us absolutely nothing about the sequences of amino acids (readily admitted by Sibley, p. 18) and are simply (and principally) a reflection of net electric charge. Differences in mobility between two homologous proteins are not necessarily proportional to the *number* of amino acid differences. It is possible that one or two differences could give as great or greater change in mobility as would ten or more differences. What seems to be important is the position of an amino acid change and the consequences this has on the configuration and charge of the molecule. A critical point not discussed by Sibley is the fact that a relatively small percentage (probably about 25%) of the amino acid substitutions result in a difference in the charge of the molecule (and thus electrophoretic mobility). Accordingly, many substitutions which are taking place are not being detected by Sibley's techniques. Second, it is particularly meaningless to speak of genetic relatedness when we are only considering *four or five genes at the most.*

The crux of the above paragraphs would seem to be that *egg-white proteins have minimal resolving power* in the determination of relationships (despite the claims of Dessauer, 1969, *Systematic Biology,* p. 327, to the contrary). By resolving power I mean the ability of characters to provide, with careful analysis, different character-states that define taxa at succeedingly lower levels of the hierarchy (this is the ideal situation and certainly not attainable in all cases). Egg-white proteins, as studied by electrophoretic techniques, cannot begin to do this. Sibley (p. 109) and some other workers are rightly skeptical of the ability of previous morphological investigations to provide us with reliable conclusions about the relationships of avian families, but they are skeptical for the wrong reasons. Contrary to their opinions (implied or explicit) I do not believe many of the previous studies presented a sufficiently rigorous analysis of their characters or were based on useful systematic theory. Many current workers are beginning to realize this and much of the old work is being re-investigated. I have no doubt that when detailed analyses of muscular and osteological complexes (which themselves can be a reflection of a large sample of the genome) are completed, we will be able to make statements about the inter-relationships of avian families that have high probability of being true. Of course other workers, including "molecular systematists", are entitled to consider this my prejudice as an anatomist who is also trying to solve some of these problems.

It is unfortunate that this review has had to be negative, but there is little to suggest that the last ten years of hard work on egg-white proteins has provided much reliable evidence on the relationships of avian families. I cannot agree more with B. G. Brehm (1969. *Systematic Biology,* p. 366) that "the future of macromolecular systematics may depend more upon the establishment of a conceptual basis for the macromolecular approach than on the solution of specific problems in the phylogeny of organisms".—Joel Cracraft.