

RICHARD J. CLARK

# RAPTOR RESEARCH



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## RAPTOR RESEARCH

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**RAPTOR RESEARCH** is published quarterly in Spring, Summer, Fall, and Winter issues and occasional Supplements. The contents are usually divided into three sections. The first section is *SCIENTIFIC PAPERS* for reports of original research or theoretical analyses. These papers will be given careful editorial and referee scrutiny. A second section, *REPORTS, REVIEWS, AND OPINION*, will include secondary material, translations of material originally published elsewhere, reports of work still in progress, reports on meetings, often in some detail, book reviews, and other similar items. This material will be edited for accuracy but will not receive the critical review given the Scientific Papers. Because of the preliminary or secondary nature of the material in this section the Editors recommend that this material be cited in other papers only with great care or in a very general way and especially with specific preliminary or conference material only after consultation with the source of that information. Papers which express a personal opinion or letters to the Editor will be included in this section. *NOTES, NEWS, AND QUERIES* is used for notices of information or events, requests for information, news items either specially prepared or reprinted from other sources, and similar small items.

This journal began publication as **RAPTOR RESEARCH NEWS** with Volume 1 in 1967 as a quarterly in typewritten mimeographed form on an 8½" by 11" page size. Volumes 2 and 3 in 1968 and 1969 were offset printed but continued the same frequency, page size, and standard typewriter type. An analytical index for Volumes 1-3 was published. Volumes 4 and 5 in 1970 and 1971 were published six times a year in offset printing, 5½" by 8½" page size, and with IBM Composer typefaces; an analytical index for Volumes 4-5 is in preparation. In 1972, Volume 6, the name of the journal was changed to reflect the broader scope to **RAPTOR RESEARCH**. Currently the journal is published quarterly by offset printing with 6¾" by 9½" page size and IBM Composer typefaces and annual analytical indexes.

For membership and publication costs see inside back cover.

# KETAMINE HYDROCHLORIDE AS AN ANESTHETIC FOR BIRDS<sup>1</sup>

by

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Because I have not been completely satisfied with the methods used previously to anesthetize birds, I have recently used ketamine hydrochloride (Ketaset—Bristol Lab.,<sup>2</sup>; Vetalar—Parke, Davis<sup>2</sup>) both experimentally and on clinical cases. The experience to date leads me to conclude that ketamine can be used safely and effectively as a general anesthetic for birds.

**Experimental Use.** At the highest dosage (20 mg per lb) recommended for cats, only mild tranquilization was induced in pigeons following injection of ketamine into the pectoral muscles. By increasing the dosage in small increments, it was found that general anesthesia could be induced in the experimental pigeons with a dose of 100 mg per lb (10 mg per 45 g). Neither a pharyngeal reflex nor salivation were apparent in the anesthetized pigeons; a palpebral reflex could be elicited. Respiratory and cardiac rates were reduced to 16 per minute, respectively (normal rates are 30 to 50 respirations per minute and 240 to 300 heart beats per minute). Laparotomies were performed on the anesthetized birds. The pigeons were anesthetized five times during a four-week period with 100 mg per lb of ketamine. There were no visible side effects nor fatalities.

**Case Examples.** 1. An adult, Red-tailed Hawk was anesthetized with ketamine (80 mg per lb) given intramuscularly to enable examination of a fractured wing. The toe pinch reflex was barely evident 10 minutes after the drug was given. The hawk blinked frequently and salivated profusely. When 0.1 mg of atropine was given intramuscularly, the salivation was reduced but not stopped. The hawk's respirations were slightly depressed and a mild tachycardia was present. The fracture site was manipulated without causing the bird any apparent discomfort. The hawk was standing three hours after being anesthetized.

2. A Green Heron (weight, 105 g) with a midshaft fracture of the tibiotarsal bone and a compound fracture of the humerus was given 25 mg of ketamine intramuscularly. Surgical anesthesia was reached seven minutes later. Both frac-

<sup>1</sup>Paper originally appeared in *Modern Veterinary Practice* 52(11)40-41, October 1971, except for notes at end of paper.

<sup>2</sup>Trade names mentioned; these are veterinary pharmaceutical products: the corresponding medical pharmaceutical products are Ketaject—Bristol Lab., and Ketalar—Parke, Davis.

tures were repaired by open reduction. Neither profound respiratory depression nor salivation was noticed during the 45 minutes taken to complete the surgical repairs. The bird was alert two hours after being anesthetized.

3. A Black Vulture (weight, 2 lb) was anesthetized with 200 mg of ketamine given intramuscularly to repair a humeral fracture. Open reduction was performed to insert an intramedullary pin. Though a palpebral reflex persisted, salivation was not noticed. The bird was standing three hours after being anesthetized.

4. A Screech Owl (weight, 100 g) with a fractured humerus was anesthetized with 10 mg of ketamine given intramuscularly. The open reduction and repair took 30 minutes. An additional 10 mg of ketamine was needed about half way through the repair as the owl began to show evidence of pain. Slight salivation and a palpebral reflex were present. The owl was standing two hours after being anesthetized.

Further investigation may be necessary to establish a safe dosage of ketamine for debilitated birds. I have injected increments of 10 mg, however, into pigeons during a 45-minute interval until satisfactory anesthesia was induced without untoward effects. Small, debilitated birds, such as those with tumors, might be given one mg per 45 g at intervals until surgical anesthesia was induced.

**Additional Notes.** The tachycardia observed in the red-tail was due to the cholinergic blocking activity of atropine. The anticholinergic affect is on the parasympathetic fibers of the vagus nerve. The vagus nerve when stimulated causes the heart to slow (bradycardia). Atropine is used in surgery to prevent bradycardia and to dry up salivary and bronchial secretions. The tachycardia was due to lack of vagal stimulation.

The only problem I've encountered using Ketaset is that 100 mg/lb seems a little high for birds of prey. I usually make an initial injection of 50 mg/lb and increments of 25 mg/lb as needed. I feel that 50 mg/lb is quite sufficient for procedures as imping, beak trimming, etc. Surgery usually requires a little more ketamine.

*(Paper received September 29, 1971; Additional notes received April 8, 1972.)*

## INJECTABLE ANESTHETIC FOR RAPTORS

by

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Generally the most effective anesthetics for birds are gases. They must, however, be administered through a special aspirator and they are expensive. Veterinarians are rarely prepared to use such equipment, and knowledgeable veterinarians are hesitant to use any of their routine anesthetics on birds. This is as it should be, since most anesthetics suitable for mammals would be fatal to birds.

For the past year I have been using Ketalar\* for taking raptor biopsies in the field for pesticide and poison analyses. This hallucinogen has been on the market for about two years and is now widely used in hospitals for infants and small children. In my work it has been used on raptors ranging in size from kestrels to Golden Eagles without ill effect.

Ketalar\*, ketamine hydrochloride, a product of Parke-Davis Company, comes in 20 ml vials containing 10 mg/ml and 10 ml vials containing 50 mg/ml. Bristol markets ketamine hydrochloride in comparable concentrations under the trade name Ketaject\*.

In the field I have found it most practical to use a graph (Fig. 1) for quick reference to determine dosages. The regression lines show the dosages to be used for the weight of the bird for the two solutions of Ketalar\*. These have been worked out by experience on test animals and in the field. Note that the graph extends only to 1000 g. If a bird weighs more than 1000 g it is only necessary to add the dosage for 1000 g to that called for by the increment over 1000 g. For example, a bird that weighs 1245 g is thought of as weighing 1000 + 245 g. With the 10 mg/ml solution of Ketalar\* the dosage would be  $1.2 + .38 = 1.58$  ml. If it is not possible to get the weight in grams, remember that one ounce equals 28.35 grams.

These dosages are the minimum necessary to anesthetize a bird for 10 to 15 minutes. If it is necessary to anesthetize a bird for a longer period of time, a double dose can be given initially or the original dose can be repeated after ten or 15 minutes. Such a dosage can be given repeatedly with no ill effects. Initially three to four times the minimal dose can be given without ill effects or prolonging the anesthetic effect for more than about an hour. Degradation products are rapidly excreted by the kidneys.

Ketamine hydrochloride produces anesthesia within five minutes when given intramuscularly. During this time there may be a period of struggle and it is recommended that restraint be used. The rates of breathing and heart beat rise slightly as the anesthetic takes effect, but after a few minutes they return to

\*Trade names mentioned; these are medical pharmaceutical products. The veterinary pharmaceutical terms are Vetalar—Parke, Davis, and Ketaset—Bristol.

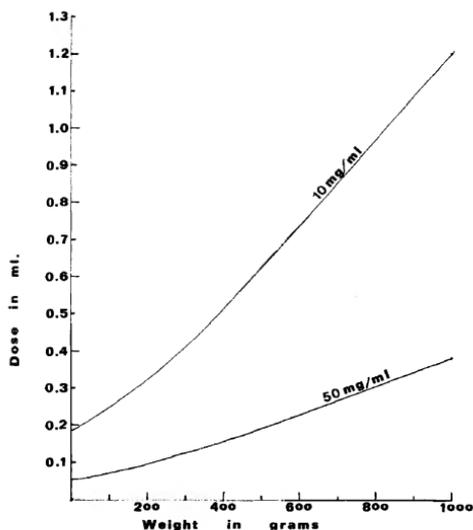


Figure 1. Intramuscular dosages of Ketalar\* for raptors.

normal. This may be deceptive as the "normal" may appear to be quite low. However, since the bird was probably very excited prior to injection, a "normal" respiratory rate and heart beat would be much slower than that observed at injection. Birds should be hooded while under the effects of this anesthetic as their eyes remain open and there is danger of damage to them.

One distinct advantage of ketamine is that pharyngeal and respiratory reflexes are maintained; thus, there is no concern about choking. This must be taken into consideration if operative procedures involve the pharynx, larynx, syrinx or bronchial tree.

A difficulty with ketamine is the long recovery period. Large birds, such as Red-tailed Hawks, may take as long as two hours to regain their coordination. Small birds, with a higher metabolic rate, recover more rapidly. Birds may attempt to fly, use their feet, turn their heads, etc., but they are not able to coordinate these reflexes for some time. Therefore, it is best to keep the bird socked and hooded with a minimum of auditory and tactile stimulation during recovery. I have been unable to determine any particular cue indicating complete recovery. When a bird stands well balanced, grabs directionally with its feet, or bites well, I consider it ready for release. Attempts to fly are poor indicators of recovery.

*(Received February 18, 1972.)*

## WEIGHING AND MEASURING RAPTORS

by

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Weights of most birds of prey can be taken using an Ohaus triple-beam balance accurate to 0.1 g with a capacity of 2,610 g. This degree of accuracy is unnecessary, particularly with larger nestlings and most adults due to interfering movements of the birds when they are on the balance. If possible, weights are taken in the morning before the first feeding. All castings (usually regurgitated in the early morning) should be weighed and these weights subtracted from the food consumption of the previous day if food consumption is being measured. The weight of pellets cast after the morning weighing should be subtracted from the morning weight.

Linear measurements are taken with a vernier caliper accurate to 0.1 mm. The blunt ends of the measuring surfaces can be filed to sharp points to facilitate approximation of the reference points used as definitions of measurements. If the limit of the vernier caliper is exceeded, the measurement is first taken using an ordinary caliper with curved arms. The length is transferred to a flat surface using the points of the vernier caliper to mark the surface. A two-step measurement is then taken with the vernier caliper. All measurements are taken on the right side of the body and recorded on a measurement data sheet.

Although definitions for standard measurements of birds do exist (Baldwin, Oberholser and Worley, 1931), the practicality of their use is questionable at times when live specimens are being measured. Not every portion of the external morphology of a live bird is linearly quantifiable because of the intervention of varying amounts of skin, plumage and muscle, which hide pertinent anatomical landmarks. Other difficulties arise due to the versatility of movement of even a restrained bird, particularly when it is necessary to place the bird on its back.

I have therefore developed a system of measurements based on practical experience gained with living specimens. All measurements were made with the bird dorsal side up to avoid unnecessary struggling and alarm. There was no purposeful disregard of established measurement definitions; rather, most are exactly the same, or nearly so, as those of Baldwin *et al.* Major exceptions include tarsal length and manus length. In the following list differences between the parameters herein defined and those normally found in the literature will be indicated as will the similarities.

(1) Thigh length (FL)—the distance from the proximal end of the femur, as defined by the groove where the head of the femur fits into the acetabulum, to

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the distal end of the femur which is indicated by the notch at the bend of the knee in which the patella lies. The groove at the proximal end of the femur is circular, so this anatomical landmark was further clarified as being that portion of the groove directly opposite the end of the femur where the groove is roughly perpendicular to the shaft of the femur. The knee is covered by skin and feathers. An effort was made to get the caliper under the feathers, but the skin and underlying tissues not actually part of the femur were necessarily included in the measurement. The measurement was made with the knee flexed and the femur lying as parallel to the trunk axis as possible, because the notch referred to above virtually disappears when the knee is extended, thereby making it difficult to standardize from measurement to measurement. Also, the underlying tissues are rearranged into a different configuration when the knee is extended, thereby introducing error. This measurement is essentially the same as that of Baldwin *et al.*

(2) Crural length (CL)—the distance from the notch at the knee to the back of the heel, i.e. to the distal end of the tibio-tarsus. Again, the measurement was made with the leg flexed as if the bird was lying down with its feet underneath its body. This method is identical with that of Baldwin *et al.*

(3) Tarsal length (TL)—the distance from the heel to the joint between the distal end of the tarso-metatarsus and the third toe. This measurement was taken to include a portion of the tibio-fibula which overlaps the tarso-metatarsus in the plane roughly perpendicular to the shaft of the tarso-metatarsus. Measuring this overlap was necessary in order to obtain a reproducible measurement. The notch between the tibio-fibula and the tarso-metatarsus was not easy to find due to the callused nature of the skin which lies over the back of the heel, particularly in young hawks which spend a great deal of time with their weight on their heels. Baldwin *et al.* do not include the overlapping portion of the tibio-fibula.

(4) Third toe length (L3)—the distance from the joint between the distal end of the tarso-metatarsus and the first phalanx of the third toe, to the point where the talon or claw emerges from the skin at the end of the toe. The toe was extended, but not stretched. The joint mentioned above was not easily found on very young hawks, but its position was estimated by bending the toe, marking the bend with a pen and then using that mark to measure the extended toe. This measurement is exactly the same as that of Baldwin *et al.*

(5) Tarsus and third toe length (LT)—the distance from the heel to the tip of the third toe obtained simply by adding the tarsal length and third toe length. This measurement is pertinent because it corresponds in an anatomical sense to the length of the manus in comparisons between fore- and hindlimbs.

(6) Leg length (LL)—the combined length of the thigh (FL), crus (CL), tarsus (TL) and third toe (L3) obtained by adding all of these measurements.

(7) Claw lengths (1C and 2C)—the distance between the point where the upper surface of the claw emerges from the skin at the tip of the toe to the end of the claw as measured across its arc. This measurement was taken as the chord of the claw as indicated by Baldwin *et al.*

(8) Brachial length (HL)—the distance from the elbow to the trunk. The

notch between the distal end of the humerus and the proximal end of the ulna on the trailing edge of the wing is well defined and was used as the reference point on the elbow. The proximal end of the brachium was probably the most difficult reference point to find consistently. The proximal or dorsal end of the coracoid articulates with the clavicle forward of the point where the humerus articulates with the pectoral girdle. At the clavicle-coracoid articulation the bones are covered with a relatively thick layer of muscle and connective tissue. There is, nevertheless, a ridge of bone near this articulation constituting the proximal end of the clavicle. This ridge was used as the proximal end of the brachium for the purpose of measurement. The humerus was held parallel to the trunk axis and the elbow was flexed when this measurement was made. When the birds were down-covered, it was very helpful to clip the down in the elbow region. Baldwin *et al.* illustrate the measurement as it was taken in this study, but their written description of the technique is sketchy.

(9) Antebrachial length (AL)—the distance from the elbow to the wrist. The notch at the elbow (defined above) was used as a point of reference for the proximal end of the antebrachium. The best reference point on the distal end of the antebrachium is a groove on the leading edge of the wrist between the radius and the carpo-metacarpus. More specifically, this is the position of the radiale bone. Again the elbow and wrist were flexed and the down was clipped in the wrist region. Except for the flexion of the elbow and wrist, this is as Baldwin *et al.* describe it.

(10) Manus length (ML)—the distance between the wrist and the tip of the third phalanx. The point on the wrist is defined above, and, in very young birds at least, the tip of the third phalanx is clearly defined. This measurement was taken *not* to include the length of the feathers as they developed. A different point of reference was adopted as the feathers emerged. The end of the third phalanx was difficult to locate at this time, but the point at which the first primary covert feather emerged from the skin closely approximated it. At the age when this feather was definitely located, the reference point was switched to its base. The objection to the method of Baldwin *et al.* is that they take this measurement on the ventral surface of the wing by placing the bird on its back. This was not found to be practical with larger birds when only one person was doing the measuring.

(11) Wing length (WL)—the combined length of the brachium (HL), antebrachium (AL) and manus (ML) obtained by adding all of these measurements.

(12) Bill depth (BD)—the distance from the culmen to the gonys in the region of the nostril. This measurement was made as a dorso-ventral perpendicular line, not as a circumference around the lateral curvature of the bill. Care was taken to completely close the mouth so that the edges of the tongue or the whole tongue did not prevent total closure. This measurement is called the height of the bill at the nostrils by Baldwin *et al.*

(13) Bill length (BL)—the distance from where the culmen emerges from the cere to the tip of the upper mandible as measured by the arc from the cere to the tip. This is equivalent to the length of the exposed culmen without the cere as described by Baldwin *et al.*

(14) Maximum width of the head (WH)—essentially the distance from one eyelid to the other taken perpendicular to the longitudinal axis of the skull. This was taken as the distance between the outer edges of the supra-orbital processes on each side, but during early development the eyelids were used since the eyes bulged out of the orbits at that time. Care was taken not to damage the eyes when measuring the maximum width of the head.

(15) Feather lengths—(a) *Total Length*—the distance from the point where the shaft exits the skin dorsally to the tip of the feather, as measured across the arc if the feather is curved and/or naturally bent. This is the length of the quill as described by Baldwin *et al.* (b) *Shafted Portion*—the distance from the point where the shaft exits the skin to the point where the protective sheath is flaking off. In the case of a fully-grown feather, the distal end of the shafted portion was defined as the point where the vanes began or, ideally, where the superior umbilicus was located. (c) *Open Portion*—the distance from the distal end of the shafted portion to the tip. The tip of the feather was difficult to define, since the natal down adhered to the tips of the major feathers. The downy tip was cut off after the feather emerged in order to allow reproducible measurements throughout the period of growth. If this was done on the day the feather was first measured, little or no error resulted. It should be emphasized that all feather measurements including covert lengths were made without straightening or flattening the feather as described for primaries and secondaries by Baldwin *et al.* Tail feather measurements were equivalent to measurements of the length of the tail as described by Baldwin *et al.*

**Sexing.** Sexing birds of prey by sight is unacceptable to most ornithologists who have not handled many raptors. Yet, experienced falconers and biologists who study birds of prey do not hesitate to sex birds of the species they know well. Those who do not appreciate the differences consider such educated guesses useless, but there are many qualitative guidelines which can be used as sex characteristics.

Most female buteos are visibly larger than males of the same species. They have larger, more massive feet, thicker tarsi and larger heads relative to the size of the body. Females usually appear broader through the shoulder region and the beak is larger relative to the size of the head. There undoubtedly is overlap in each of these features, but when the sexes are seen together, or after some experience, even nestlings can be sexed a high percentage of the time.

## Reference

Baldwin, S. P., H. C. Oberholser, and L. G. Worley; 1931. Measurements of birds. *Sci. Publ. Cleveland Mus. Natur. Hist.* 2:1-165.

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# ON PHOTOPERIOD AND CAPTIVE BREEDING OF NORTHERN PEREGRINES

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## *Introduction*

With Peregrine Falcons (*Falco peregrinus*) of northern origin there has been a general lack of success to date in efforts at captive breeding. It seems apparent from the lack of egg-laying that northern birds will not come into full reproductive condition on natural daylengths of middle latitudes where a number of them have been held for some time. Some captive pairs have shown very good indications of incipient breeding condition. From these cases it is fairly obvious that northern birds in captivity require manipulation of the daylengths to which they are exposed to something much closer to the wild condition.

The RRF Captivity Breeding Conference provided a basic discussion of photoperiod (daylength) and photoperiodic responses in birds. The following discussion will not provide an in-depth analysis of the causes and effects of the photoperiodic response which many birds show, or the internal mechanisms involved. It will provide the following:

1. Some basic information on the timing and location of breeding, migrating and "wintering" northern Peregrines;
2. An outline of the daylengths which wild northern Peregrines experience through the year; and
3. A table of daylength changes which can be followed through the year in order to give captive birds photoperiods comparable to those to which the wild birds are exposed. Only in certain parts of this discussion will any effort be made to differentiate between *F. p. tundrius* and far north *F. p. anatum* birds; in general there is too little known concerning the dates and distances of migration of northern Peregrines.

## *Some Photoperiodic Experiments with Other Species*

In order to provide a basis for discussion of the northern Peregrines and their photoperiods, I will attempt at the outset to outline some of the published experimental results of photoperiodic stimulation of other species, especially those experiments which seem to relate most directly to the northern migrant Peregrines.

By using long daylengths (with artificial lighting) to resemble summer days and short daylengths to resemble winter days, many people have caused a wide variety of bird species to come into breeding condition at various times of the

year. Migratory species (at least) when placed on long daylengths put on weight—develop their fat deposits (Wolfson 1959)—presumably in preparation for migration. The length of time it takes for the birds to come into breeding condition depends on a number of factors (light intensity, species, etc.). While the testes of males will develop fully in response to long days, the ovaries of females will not develop fully solely due to photoperiodic stimulation. For full ovarian development females, apparently, also require “psychic” factors—nesting material and/or a nesting site and, perhaps, a photoactivated male also (Farner 1959).

Many species respond to the rather long days of a natural spring by breeding. However, before the longest day of summer is reached, their gonads have regressed and they have lost their ability to breed until the following year (see Schwab 1970: Fig. 1). The rather long spring days “turn on” the birds; the even longer days near June 21 appear to “turn them off.” Various researchers have shown that by holding the birds at a certain daylength, they can be kept in breeding condition for many months. Male Starlings (*Sturnus vulgaris*) from California held on daylengths of 10½ or 11 hours remained in breeding condition “for at least 15 months” (Schwab 1970). With 12 hours of daylight they came into breeding condition and then passed through it in a couple of months, as in the wild. That is, the longer day apparently caused their breeding cycle to eventually end, whereas slightly shorter daylength caused the birds to remain in breeding condition for over a year (until the experiment was ended). The daylengths required for this response seem to vary with latitude (where the birds originated) and with species.

Some species, when held for many months on winter (short) daylengths, have eventually started to come into breeding condition. This response has led to the suggestion that birds summate or “store up” something from daylengths. Apparently, when they have “stored up” enough, they will come into breeding condition. This is the “summation hypothesis.”

The period of *darkness* each day may be as important as the period of light. A number of experiments have shown that a long night (short day) which is broken by a period of light (even a short period of light) can affect the bird in the same way as a short night (long day).

When the birds have come into and passed out of breeding condition in early summer, keeping them on the long daylengths of mid-summer will prevent them from coming into breeding condition again, even a year later. After the gonads have begun to regress in early or mid-summer, the birds require a “rest period” of shorter days—a *photorefractory period*—before they can again come into breeding condition.

The refractory period has some peculiarities. If long days are introduced during the refractory period, in effect the refractory period starts all over again, i.e. the refractory period must be an *uninterrupted* period of short days (Wolfson 1959).

Farner (1959) has commented on autumnal sexual activities sometimes seen in birds, particularly in resident species. He suggests that the refractory period has ended relatively early (August–September) in these birds, while days are

still long enough to cause appreciable stimulation of the birds. Sexual activities in the autumn apparently vary from year to year depending on weather conditions.

Farner (1954, cited by Engels 1959) suggests that for North American species which winter south of the equator, the refractory period may be rather long, ending sometime after the South American summer solstice (December 21). The birds could then be triggered to migrate northward by the still long (though shortening) days. Engels (1959) found that Bobolinks (*Dolichonyx oryzivorus*) (breeding 40-50 degrees N, "wintering" 15-25 degrees S) possess a photorefractory period. Birds held on short ("winter") days for eight weeks, then placed on 14-hour ("summer") days, came into breeding condition more quickly if the "winter" (refractory period) days were 10 hours instead of 12 hours long. Shorter "winter" days appear to hasten the end of the refractory period.

Most experiments with photoperiod appear to have been conducted with sudden changes from one daylength to another (e.g. from 10 hours suddenly to 14 hours). This has probably been done more as a matter of convenience than a result of experimental design. However, as will be pointed out below, it appears that from *gradual* changes in daylength more can be learned and some particular problems may be avoided. At the moment, in attempting to breed raptors, we must be primarily concerned with achieving successful reproduction. Experimentation into the causes and internal mechanisms can come later. Wild birds respond to *gradual* changes in photoperiod and it should be expected that they will also respond best to gradually changing photoperiod in captivity.

### *Annual Cycle of Northern Peregrines*

For a northern Peregrine the year is broken into four distinct periods: migration south, "wintering," migration north, and breeding. It is with the breeding season that we are primarily concerned in captive breeding, but the other periods of the year are probably equally important in setting the stage for breeding.

In Figure 1 some of the available data on migration and "wintering" of northern Peregrines are summarized. Only the more important points will be considered here.

1. **Departure south.** White (1969) noted movements of Peregrines in Alaska during the first two weeks of September. Shor (1970) mentioned an adult Peregrine shot in Greenland on 4 September 1958 north of the Arctic Circle. From this it can be suggested that the birds might be beginning the flight south in the first or second week of September.

2. **Migration south.** In Wisconsin most migrating Peregrines are observed between 22 September and 5 October (D. Berger, cited by Enderson 1965). Berry's (1970) data suggest that the majority of falcons pass the Virginia coast in the period 28 September–12 October (or slightly later). On the Texas coast, from Enderson's (1965) and other data, the main portion of the migration appears to occur about 5-25 October. Enderson's data indicate that it takes about

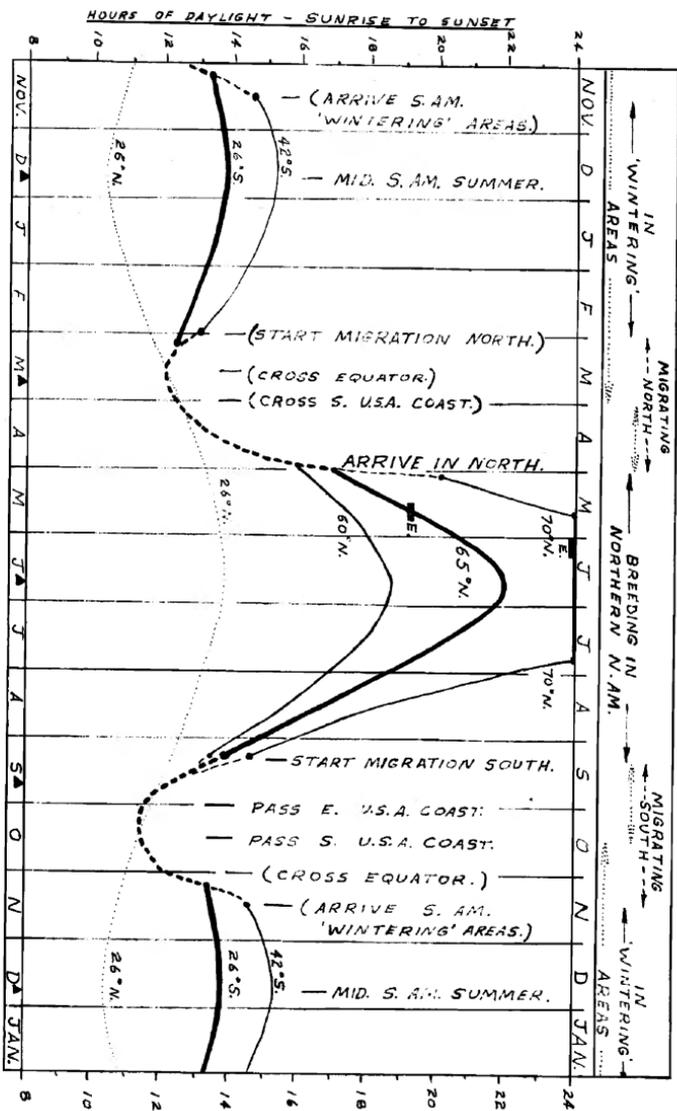


Figure 2. Approximate annual cycle of daylength (sunrise to sunset) to which a Peregrine breeding in northern North America is exposed. [Dashed lines indicate migration. Dotted line shows yearly photoperiod at Miami, Florida. Parentheses show uncertainties due to insufficient data. Solid triangles indicate solstices and equinoxes. E = egg-laying (from Cade 1960). Photoperiod data from Thomas (1953:181), Robbins *et al.* (1964:332), and U. S. Naval Observatory (1969:386-393).]

two weeks for a falcon to fly from Wisconsin to the Gulf Coast. By using this time and distance with Figure 1 we can roughly predict when the falcons will reach regions farther south. Birds wintering between the Tropic of Cancer and the equator probably migrate for just over one month. Falcons flying to south of the Tropic of Capricorn probably require just over two months.

3. **“Wintering” areas.** In Figure 1 band recoveries from migrant Peregrines taken in “winter” (November–February, the South American summer) are plotted from the data of Shor (1970) and Kuyt (1967). By specifying the period November–February I have eliminated a number of banding records which might have been from birds which were migrating (e.g. in October, March and April). Data from birds banded in Wisconsin (Enderson 1965) suggest less of a movement of that population to the southern parts of South America than do data from birds banded on the east coast.

Shor’s (1970) data from east coast birds suggest that immatures and adults do “winter” in the same regions. There is a hint in his data that falcons which pass the Virginia coast earlier in the season migrate to “wintering” areas farther south. Birds banded before about 8 October are heading south of the equator, and those passing after about 8 October winter between the Gulf Coast and the equator.

White (1968) points out that Peregrines in North America exhibit “leapfrog” migration. Those breeding in the far north tend to winter farthest south. There is also a tendency for smaller birds to winter farther south. Kuyt’s (1967) record of a banded nestling Peregrine (approximately 64 degrees N) being killed in Argentina supports this (Fig. 1). White (1968, citing Hunt 1966) further points out that falcons wintering near the Gulf Coast appear to be western or taiga birds, rather than pale *F. p. tundrius*. Darker taiga birds (see the tree-line indicated on Fig. 1) appear to “winter as far south as Central America and occasionally northern South America.”

4. **Migration north.** Meredith (cited by Enderson 1965) observed wintering “arctic” Peregrines (presumably taiga birds) in south Texas until March. Other than this, there appears to be very little known about the northward migration of northern Peregrines. If we speculate that the flight north takes as long as the flight south, then birds in the southern part of the “winter” range would have to start north in late February or early March.

5. **Arrival north.** Cade (1960) and White (1969) indicate that Peregrines arrive in the Alaskan interior in very late April or early May, and on the Arctic Slope in the first or second week of May.

6. **Breeding season.** In the Alaskan interior, Cade (1960) noted one instance in which the first egg appeared about May 13. He estimated egg-laying to occur mostly during the third week of May in the interior (about 65 degrees N) and during the first week of June on the Colville River (Arctic Slope, about 69-70 degrees N). This would be about two to four weeks after arrival in the area. These falcons have only about four months—a rather short period—in which to carry out all their breeding activities.

*Photoperiodic Changes in the Wild*

The curves in Figure 2 are derived from the migration data in Figure 1 and from various sources of information of photoperiod. Latitude 42 degrees S appears to be at the southern extreme of the "wintering" range of North American Peregrines in South America.

The heavier line in the center of this figure illustrates the changes in photoperiod to which a Peregrine breeding at 65 degrees N is exposed through the summer. At this point, a difficulty arises: which birds winter where? There apparently is a difference between the migration patterns of *F. p. tundrius* (tundra) and northern *F. p. anatum* (taiga) (White 1968). A falcon from 65 degrees N could be of either subspecies.

1. If it is *F. p. tundrius* from 65 degrees N, then its annual photoperiodic cycle would most likely be matched by the heavier lines of Figure 2—26 degrees S in "winter" and 65 degrees N in summer. The nestling recovery shown in Figure 1 suggests this, also.

2. A taiga (*F. p. anatum*) falcon from 65 degrees N might winter as far south as the equator, and probably at, or south of, the Gulf Coast. In this region the annual photoperiodic changes are slight, and the "wintering" falcon would be experiencing days in which the sun is up for about 10½ to 12 hours, following, or slightly above the curve for 26 degrees N (Miami, Florida). Its period of migration north and south would likely be somewhat shorter than indicated by the dashed migration periods in Figure 2, since it would not have to fly to south of the equator. The "wintering" period would be slightly longer.

3. A falcon breeding on the Arctic Circle (66½ degrees N) would see the sun for the full 24 hours of the day only on June 21. Birds north of the Arctic Circle experience more days with 24-hour sunlight, depending on how far north of the Arctic Circle they are.

Since it appears that most northern falcons arrive in the breeding areas about the first week of May, a bird breeding north of the Arctic Circle would have spring and summer daylengths indicated by curves steeper than those shown for 65 degrees N and resembling more the curve shown for 70 degrees N. Birds breeding north of the Arctic Circle would arrive in the spring when days are 19 or more hours long, having flown north through a period of rapidly lengthening days. (The steepness of the curve indicates how rapidly daylength is changing).

Since falcons breeding farthest north can be expected to be "wintering" farthest south, the curve shown for 42 degrees S probably represents the "winter" photoperiod of birds from higher latitudes of the arctic (e.g. 70 degrees N).

As Figure 2 shows, once the bird has arrived in its northern breeding area, the rate of daylength increase slows somewhat from the rate during the migration north.

During the longest day of the South American summer (December 21), birds at 42 degrees S receive only 1½ hours more daylight than birds at 26 degrees S (about 15¼ and 13¾ hours of sunlight, respectively). By way of contrast, on December 21, falcons "wintering" at Miami, Florida, or Brownsville, Texas, would receive only 10 1/3 hours of sunlight, this being the *shortest* day of their

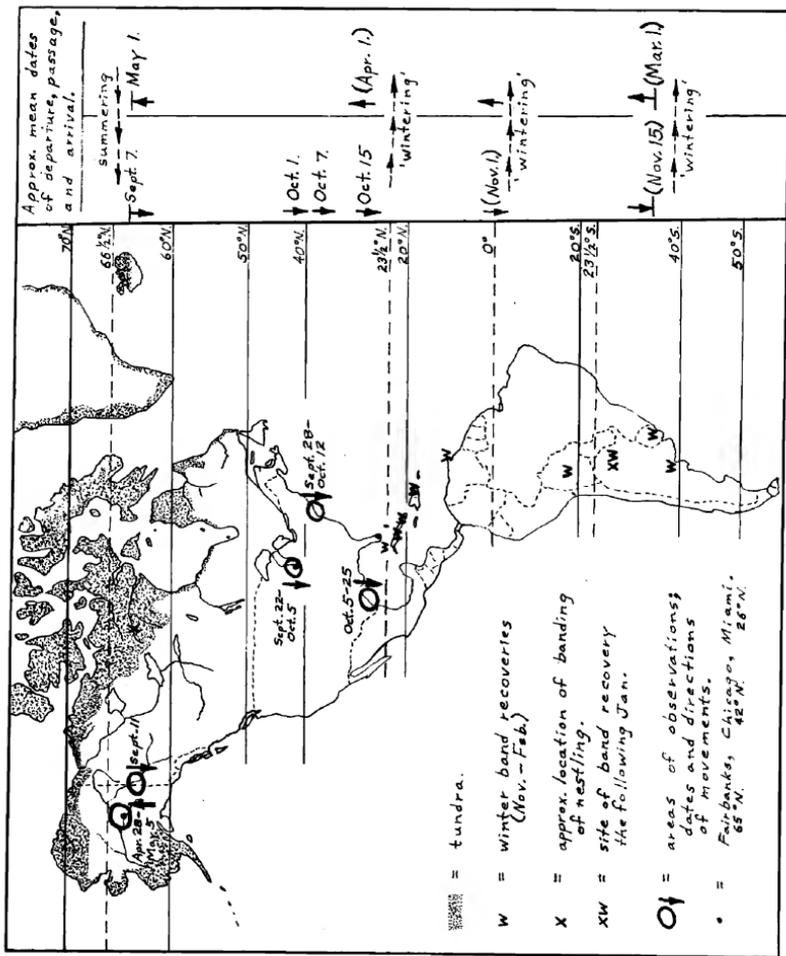


Figure 1. Winter band recoveries and migration dates of northern breeding Peregrines. [Banding data from Shor (1970) and Kuyt (1967). Migration schedules from (S to N) Enderson (1965), Berry (1971), Berger (cited in Enderson 1965), and White (1969).]

year.

Twilight must be considered, if only briefly. At 65 degrees N (Fairbanks, Alaska) the twilight extends through the night "so the city will not become even dark enough to require street lights or automobile headlights. This condition prevails each year from about May 15 to approximately July 27" (Nicholson 1963). The effect of this upon falcons (or any birds in which photoperiod has been studied) does not appear to be known.

### *Photoperiodic Cycles for Captive Northern Peregrines*

Table 1 shows the approximate annual cycle of changes in daylength (sunrise to sunset) to which falcons from northern regions are exposed, based upon the data in Figure 1 and Figure 2. This table, if applied to northern Peregrines in captivity by means of artificial lighting (incandescent or fluorescent), should trigger these birds into breeding condition or, at least, bring them very much closer to it, especially if other factors are also conducive to breeding (i.e. the birds are undisturbed, food and water are plentiful, lighting is reasonably bright, adequate perches and ledges are provided, etc.).

At the outset, a decision must be made as to where the birds in captivity might have originated in the north. (Eyass-taken birds will not have this problem, although they may have imprinting problems.) On the basis of the migration data provided and the coloration of immature and adult plumages (see White 1968 for subspecies characteristics), it should be possible to make an informed guess as to the subspecies and latitude of origin. In cases of real difficulties or overlap, a means of solving the problem might be to follow the schedule of photoperiods for *F. p. tundrius* from 65 degrees N and, if required, gradually continuing beyond 22 hours of light towards 24 hours of light to discover when the birds come into prime breeding condition.

For reasons which will be pointed out below, in Table 1 the changes in daylength are *gradually* brought about with daily increases or decreases being provided, and the northward migration and northern spring periods for the captives are slightly lengthened. It will probably be possible to bring birds into breeding condition by first giving them two to three months on "winter" daylengths, then "migrating" them north with changing photoperiods; Table 1 begins with the "winter" photoperiods for this reason. It may not matter whether the daylength changes (additions or subtractions) are applied half at the beginning of the day and half at the end of the day, on alternate days at the beginning and end of the day, or consistently at either end of the day. There is some evidence, however, that the safest course is to make the changes equally at both ends of the day (see Meier *et al.* 1971 for some endocrinological evidence). By arranging the photoperiodic schedule ahead of time, it would be possible to have the beginnings of the changing days at convenient times for feeding and observing the birds.

The brightness of the quarters may have an effect upon the timing and schedule of breeding activities. Birds in the wild experience cloudy days, whereas captive birds under lights always have the "sun" present. It is possible that captive

**Table 1.** Photoperiod schedules proposed for use in captive breeding of northern Peregrines. [The columns indicate the number of weeks during which changes in photoperiod are brought about. The comparable situation in the wild is shown by the location of the wild birds and the approximate dates during which they experience similar photoperiods. (e.g. in Stage 2, start at 13½ hours of light per day; subtract one minute of light each day, to arrive at 12 1/3 hours of light in 10 weeks time. This is comparable to “wintering” at 26 degrees S, from December 22 to February 28.)]

Stage	Duration, in Weeks	Photoperiod Changes (add or subt. min./day)	In the Wild	
			Location	Month
<i>tundrius</i> breeding at 65°N, “wintering” at 26°S				
1.	6	13½ hrs. (no change)	26°S	15/11-21/12
2.	10	13½→12 1/3 hrs. (-1)	26°S	22/12-28/2
3.	2	12 1/3→12 hrs. (-½)	Migr. N	1/3-14/3
4.	7	12→17 hrs. (+6)	Migr. N	15/3-30/4
5.	8½	17→22 hrs. (+5)	65°N	1/5-21/6
6.	13	22→11½ hrs. (-7)	65°N & Migr. S	22/6-30/9
7.	1½	11½ hrs. (no change)	Migr. S	1/10-14/10
8.	4	11½→13½ hrs. (+4)	Migr. S	15/10-14/11
Proceed to Stage 1.				
<i>anatum</i> breeding at 65°N, “wintering” at 26°N				
1-2.	10½	11 hrs. (no change)	26°N	15/10-28/2
3.	2	11→12 hrs. (+4)	26°N	1/3-14/3
4.	7	12→17 hrs. (+6)	Migr. N	15/3-30/4
5.	8½	17→22 hrs. (+5)	65°N	1/5-21/6
6.	13	22→11½ hrs. (-7)	65°N & Migr. S	22/6-30/9
7-8.	2	11½→11 hrs. (-2)	Migr. S	1/10-14/10
Proceed to Stage 1.				
<i>tundrius</i> breeding at 70°N, “wintering” at 42°S				
1.	6	14½ hrs. (no change)	42°S	15/11-21/12
2.	10	14½→13½ hrs. (-1)	42°S	22/12-28/2
3.	2	13½→12 hrs. (-6)	Migr. N	1/3-14/3
4.	7	12→20 hrs. (+10)	Migr. N	15/3-30/4
5a.	4	20→24 hrs. (+8)	70°N	1/5-21/5
5b.	9	24 hrs. (no change)	70°N	22/5-21/7
6.	8½	24→11½ hrs. (-12½)	70°N & Migr. S	22/7-30/9
7.	1½	11½ hrs. (no change)	Migr. S	1/10-14/10
8.	4	11½→14½ hrs. (+7)	Migr. S	15/10-14/11
Proceed to Stage 1.				

birds may respond slightly earlier on their schedule of lighting, *if* it is roughly equivalent to the light intensity outdoors. If indoor lighting is of low intensity, the birds will probably come into breeding condition somewhat late, and the male will probably be unsynchronized with the female (see Koehler 1969).

### *Some Captive Breeding Efforts with Northern Peregrines*

1. In January 1967 Fyfe (BPIE 4) put a pair of eyass, intermewed, northern Peregrines into new quarters and onto artificial light (100-watt bulb). The birds were disturbed only at feeding time and were observed through a peephole. The lighting regime was as follows: Approximately three months on 12 hours of light, then increases of one hour of light per week until 24 hours of light was reached, then kept at 24 hours of light.

At about 15 to 16 hours of light per day the birds "become more noisy and aggressive, particularly the tiercel at the nest ledge" and they spent much time flying back and forth—"migrating." Both lost some weight. This activity slowed when they reached about 22 to 23 hours of light per day. While on 24 hours of light, the female spent much time on the nest ledge and was seen to sleep lying on her breast. Both birds became quieter and they molted normally.

There may be a number of reasons why this pair apparently just missed reproducing (e.g. intrusion into their territory at feeding time), but the consideration of the photoperiod provides some other good suggestions.

The "migration," noise, and aggression apparently began when the birds were on 15 to 16 hours of light. Compared to Figure 2, this photoperiod indicates the latter part of the natural migration phase. From this we might presume that (1) the pair, having the "migration" restlessness, tried to migrate or, at least, to "burn off energy" due to the changing photoperiod, and (2) being confined as a pair, with ample food and a nest ledge, they came into territorial behavior and incipient breeding condition somewhat earlier than might be expected in the wild.

The female of this pair originated at about 63 degrees N; the male was from somewhere in Alaska. The case of the female may be most important. At 63 degrees N the female would never see a 24-hour day; the longest day (June 21) might be about 21½ to 21¾ hours long from sunrise to sunset (with twilight through the night).

Just as photoperiod can act as a trigger to start breeding behavior and gonadal development, it appears (experiments discussed previously) that it also causes the birds to *stop* gonadal development and to stop production of eggs at a time when (even though the days are still getting longer) it would be impossible to rear young to independence before migration south must begin. From Figure 2 it can be seen that even birds at 70 degrees N lay eggs well before June 21. Most likely the birds are triggered to lay by the photoperiod a week or more *prior* to actual egg-laying.

In this instance with a nearly successful captive pair, is it possible that the photoperiod was increased *too quickly* (one hour per week) and *too far* (to 24 hours of light) so that the female was suddenly "turned off" by the very long

days *before* she was sufficiently “turned on” (gradually) by the shorter days?

(In birds from southern regions, i.e. southern-*breeding* falcons, 24-hour light might provide stimulus for successful breeding. Such birds have a much longer summer and may not need a rapidly-acting “shut-off” mechanism like the birds in the north with a shorter breeding season.)

2. Berry (BPIE 18) described the behavior of his adult passage tundra Peregrines on Pennsylvania photoperiod: “Last fall, both birds very noisy, often facing one another with head down, tail elevated. Inspection of open air shelf disclosed a scrape . . .” Grainger Hunt (pers. comm.) has observed a 2½ year old passage tiercel come into courtship behavior in early December on Texas photoperiod. Why in the fall? There may be two (or more) possible explanations for this.

First, it was mentioned earlier that some birds (male juncos) start coming into breeding condition when held for a long time on short days. Tests with females on continuous winter daylengths do not appear to have been conducted in the same way to see if they would eventually come into breeding condition. In any case, it has been hypothesized that the triggering of migration and breeding condition may be the result of the *summation* of light striking a bird over a period of time, rather than the ratio of light to dark or the strict length of day. If this is the case, then northern falcons might be expected to come into some form of breeding condition at any time of the year, when they have “stored up” the appropriate amount of light stimulation.

Second, another possible explanation for the *fall* activity of some northern Peregrines held at middle latitudes concerns the triggering mechanism for the “spring” migration north. From Figure 2 it can be seen that Peregrines “wintering” south of the equator *begin the migration north in the autumn*—the South American autumn with its *shortening* days. Only when they near the equator will they come into spring and its lengthening days. It seems possible that these captive birds are reacting to the daylengths which trigger migration (shortening days) and, being confined with plentiful food and with ledges, the territorial and courtship behavior can immediately follow. Such birds will not reach full breeding condition due to lack of the triggering effect of rather long northern spring days. In these cases, “flying the birds north” with lengthening days (under lights) may be sufficient to stimulate them into full breeding condition; the shortening days will have started the process and lengthening days could complete it.

### *Other Considerations*

1. **Cold winters and very short days.** By holding northern Peregrines overwinter in mid-latitude North America we are causing them to experience very short days, much shorter than they ever see in the wild. Judging by experiments on other species, shorter than natural daylengths appear to be of no major consequence to subsequent breeding seasons. However, keeping these birds overwinter in very cold weather might be pushing things a little too far. The northern birds, while they will tolerate prolonged exposure to zero or sub-zero weather

er, certainly never experience such hardships in the wild. Their migration patterns indicate that they go to great lengths to avoid just such temperature extremes. By keeping them in *warmed* quarters they could be kept away from any inhibiting effect extreme cold might have on them. Under such conditions the 12-month cycle could be carried out at any time of the year, with birds breeding in December if one wished.

**2. Migration and wintering of the sexes.** While it is possible, it does not seem likely that a pair of falcons would migrate and “winter” together. There appear to be no migration records suggesting this. Until experiments are carried out with birds separated during the winter and re-introduced in the spring, versus birds kept together year-round, we will not know if there are any harmful or beneficial effects of either means of “wintering” the birds. It is possible that re-introduction of the pair in the spring (at the appropriate photoperiod) could be very stimulating to the birds.

**3. Twilight.** In the spring, at least, there might be some benefit derived from giving falcons a twilight period, particularly in the evenings. By allowing a small shaded (not glaring) bulb (e.g. 25-watt) to burn for a period of time in the evening, it would allow the birds access to their night perches without being struck suddenly with darkness. In the breeding season such a small light might be left on throughout the whole day to provide a nighttime twilight as in the wild.

**4. Gradual versus sudden changes in photoperiod.** Much of the experimental work with smaller birds, and some work with raptors (e.g. Willoughby and Cade 1964), has been done by suddenly giving the birds longer days (say, 18 hours of light) when breeding is desired. From the discussion above it appears possible to “turn off” breeding of northern birds by initiating a long daylength too quickly, especially if the daylength is much longer than that which birds from that particular birthplace would normally receive. Even if the birthplace of a pair of birds is known, we still do not know (yet) what the triggering photoperiod is for shutting off egg-laying. The photoperiod thresholds may be reached and the birds “turned on” or “turned off” *weeks* before we can see the effects.

Another reason for gradually taking the birds into the lengthening photoperiod of the northern spring (as is done in Table 1) is that we simply do not yet know where the trapped migrant falcons in captivity originated. By bringing the trapped migrants through a “winter” period of lighting (e.g. 26 degrees S if the birds appear to be *F. p. tundrius*), then taking them into the photoperiod changes associated with the “migration north” until they reach 16 hours of light per day (see Fig. 2), and then gradually “taking them north” by gradually lengthening the days, it should be possible to determine the exact photoperiod at which the birds come into maximum breeding behavior and then into egg-laying. Compared to a bird from 60 degrees N, one which originated at 70 degrees N would most likely require a considerably longer daylength in order to come into egg-laying. Gradually changing the daylengths may suggest where the falcons originated in the north.

Further, by *gradually* bringing the birds into the breeding photoperiod (by

increases of about five to six minutes per day), and once having a clutch, it may be possible to obtain replacement clutches from the northern falcons simply by *keeping* them at the egg-laying photoperiod for a month or so before taking them back into the “natural” changes toward mid-summer daylengths and, later, the autumn. By holding them at the egg-laying photoperiod it should be possible to prevent them from reaching the “turn-off” photoperiod of a few weeks later in the “natural” summer. If the breeding season is extended in this way, then the “annual” regime could still be kept to 12 months by (1) eliminating Stage 7 in Table 1, (2) shortening the “winter” period slightly or (3) speeding up the period of the southward migration so that it takes less time to get to the “winter” photoperiod. Too much shortening of the “winter” may eliminate the required photorefractory period. Sudden increases in daylength pose the probability of over-shooting the triggering photoperiods and thereby preventing egg-laying. Much more could be learned from gradually increasing the daylength.

In a captive pair of falcons which was trapped on migration it is possible that the male and female originated from widely separated latitudes and that they would come into maximum reproductive condition at different photoperiods. If a problem such as this arises, three courses of action might be open: (1) keep the birds separated and on differing light regimes so that they would be synchronized for successful artificial insemination in the next breeding season, (2) attempt to synchronize them together by rather quickly increasing the photoperiod in the breeding season to the stimulating daylength of the slower (longer daylength) individual so that one might still be in breeding condition when the other just arrived in breeding condition, or (3) pair the birds with other birds which have responded to more comparable photoperiods to increase the likelihood that they were from similar latitudes.

**5. Observing and recording behavior and photoperiods.** This subject has been considered earlier (RRF Conference Behavior Panel Report), but with northern falcons there are some particularly crucial aspects.

From the preceding discussion it is obvious that responses of birds to “northward migration” and “spring” and “summer” photoperiods must be closely watched; otherwise it will be impossible to determine the causes of failures of pairs or to determine the photoperiods at which pairs did lay eggs. It cannot be emphasized too strongly that records (descriptions of behavior alongside the photoperiod of that date) must be taken so that (1) behavioral changes can be related to the photoperiods at which they occur, (2) the photoperiods and behavioral changes of unsuccessful pairs can be compared with those of successful pairs and (3) solutions to these problems may be arrived at as quickly as possible.

### *Conclusions and Summary*

1. The lack of egg-laying by Peregrines of northern origin which are held in captive breeding situations suggests that these birds require photoperiodic manipulation in order to be induced to breed in captivity. The long daylength of the

northern spring is thought to be a necessity for egg-laying to occur.

2. The studies of Peregrine subspecies and of Peregrine migration (banding and censusing), fit together to suggest the photoperiodic changes that wild falcons experience through the year:

a. Northern *F. p. anatum* (taiga) falcons appear to "winter" north of the equator and presumably experience "winter" daylengths of 11 to 12 hours.

b. *F. p. tundrius* falcons appear to "winter" in South America, as far south as about 40 degrees S. They experience a second summer from November through January and begin migrating north during the South American autumn. Daylengths of the South American summer do not exceed about 15 hours within the migrant falcons' range. This is well below the suspected triggering daylengths for egg-laying.

3. The photoperiod which triggers egg-laying most likely varies depending on the latitude at which the birds originate; birds from farther north will require longer photoperiods in order to be stimulated sufficiently to breed.

4. Rather than giving the birds a sudden change from a short (winter) day to a long (summer) day, a gradual change over a number of weeks is recommended. A number of important advantages of gradual changes in photoperiod are pointed out.

5. When manipulating photoperiod, the light cycles must be followed very faithfully. Some precautions must be taken to ensure that the birds are stimulated by light only in the manner desired:

a. When birds are in the night period, their lights should *not* be turned on, not even for a matter of minutes. This could duplicate "long days" and destroy the photoperiodic schedule by advancing the birds ahead or prolonging their refractory period.

b. When in the refractory period of the year, birds must be subjected to *no long days*. Long days could cause the refractory period to start all over again.

c. Twenty-four hours of daylight may *inhibit* egg-laying in birds from below or near the Arctic Circle. If birds are known or suspected to be from this region, they should be taken rather slowly through shorter daylengths before being brought near to 24 hours of light as a last resort.

6. A table is provided which allows captive birds to be given rather close approximations of the photoperiods they would receive in the wild. Barring other interferences such photoperiodic manipulation should bring northern falcons into breeding condition in captivity.

7. By carefully observing and recording the behavior of the photostimulated falcons, it should be possible to determine the photoperiods at which they are stimulated to show breeding behavior and to lay eggs. By recording the behavior and photoperiod at which it occurs a number of captive breeding problems would appear solvable.

8. Separation of the sexes during "winter", and "wintering" birds in "non-breeding rooms" may be helpful to the breeding of migrant birds. They could be re-united and/or allowed access to the breeding room at the appropriate photoperiod in the "spring." Both of these are conditions which apparently

occur in the wild about two to four weeks prior to egg-laying and both are likely to be very stimulating to the birds.

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# EXPERIMENTS ON CAPTIVE BREEDING AND PHOTOPERIODISM IN PEREGRINES AND MERLINS

by

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## *Introduction*

This paper was submitted to *Raptor Research* to complement R. W. Nelson's paper "On Photoperiod and Captive Breeding of Northern Peregrines" (this issue) which gives background material on photoperiodic principles. Little will be said concerning principles in this paper; the reader is referred to the preceding paper and to Willoughby and Cade (1964).

For reasons outlined in Nelson's paper, it seems possible that artificial manipulation of the photoperiod of captive taiga or tundra Peregrines will be necessary for successful breeding. Furthermore, it is likely that these manipulations will have to mimic the natural experience of the wild birds to at least some degree.

While it would be far better "science" to proceed in an orderly fashion towards the "full" explanation of photoperiodism in migratory falcons, as well as the behavioral, nutritional and other factors influencing breeding, I believe, as Nelson does, that our primary task lies in somehow achieving breeding. "Experimentation into the causes and internal mechanisms can come later" (Nelson, this issue). Unfortunately, it is obvious that we need information on physiological and behavioral mechanisms to achieve breeding. It was with this in mind that I began efforts both to achieve breeding and to conduct scientific experimentation simultaneously—an uneasy fusion of science and art and not a situation entirely compatible with either. The inadequacies of the work in sample size, lack of controls and experimental design will be obvious.

## *Methods and Rationale*

In 1967 I began experiments with northern Merlins believing that attempts to breed the more difficult species, such as Peregrines, were appropriately preceded with knowledge gained from a species intermediate in some behavioral, evolutionary and morphological aspects between these large falcons and kestrels. The latter are almost routinely bred in captivity. These experiments have been continued to the present. In 1969 work began with northern Peregrines and is also continuing.

The experiments have been conducted with the following basic assumptions in mind:

1. Photoperiod is the key element in producing a physiological state in which

breeding will occur. In regions in which there is little difference in photoperiod between seasons, rainfall or other factors may be dominant; this is certainly not true of the birds used in these experiments.

2. The passage of *time* as such is not important; what is important is the completion of a series of physiological and behavioral events that lead to the state we desire (sexual maturity first, then breeding). This, of course, would include the "winding down" or termination of the breeding state before another series begins. It seemed possible, therefore, that as long as there existed "spare time" in the "physiological year" of the bird, that both sexual maturity and the number of breeding cycles per year might be accelerated. This presupposes that there is no fixed endogenous annual periodicity (see Immelman, 1971, for summary material), a point of some insecurity with the birds in question. I have, therefore, carried on most of the experiments at a pace considerably accelerated from that normal to the species in the wild.

3. In most species of middle or high latitude birds a refractory period exists following breeding during which the bird will not respond to increased day length. There are no direct data on this point concerning Peregrines or Merlins, and I have assumed, largely from data on passerines, that an allowance of approximately three months for completion of a refractory period would be reasonable.

4. While it is possible that subtle environmental factors might conceivably be significant in breeding, I have chosen to keep the experiments as simple and well defined as possible and, with the exception of allowing the Peregrines to have a single, glassed window during the last breeding attempt, I have largely excluded visual and thermal components of the natural environment.

5. I have also assumed, apart from issues of pair compatibility, that behavioral issues are secondary in origin and significance to physiological ones and will take care of themselves once the physiological sequences have been set in train. Obviously, one can carry this sort of logic too far, but I have attempted to provide at least minimal room for the birds within the rather severe strictures imposed by living in Fairbanks, Alaska, and as experience is acquired, I am modifying those features of the experimental setup which seem to adversely influence behavior.

### *The Birds*

Four Merlins were used. Two downy young, which ultimately turned out to be two females, were taken from a ground nest at 65 degrees 30 minutes N in 1967. This duo was used in the first series of experiments. A male and female were taken from an old magpie nest at 64 degrees 30 minutes N at about four and one-half weeks of age in 1969.

Three Peregrines were used. One female was taken in 1967 in the taiga zone at about 65 degrees N at four and one-half weeks of age and was flown in falconry during the summers of 1967 and 1968. This bird was maintained in a mews with windows on "natural" photoperiod until the fall of 1968 when all openings were closed and experimental photoperiods began. A male and female,

approximately four weeks old, were given to me in 1969 and are of unknown origin. It is probable that their nest lay in the taiga between 65 degrees and 66 degrees N and that they are siblings. These birds were immediately placed in quarters from which they have never been moved and hence may have been together since they hatched. They have not been handled. The female is bold and behaves toward me in the manner often attributed to an "imprinted" bird. The male is shy in the presence of humans, but as a pair no signs of friction or undue dominance by one member has been seen.

### *Housing*

Merlins have been housed in two different rooms, both in the basement of my house. Both rooms measure approximately ten by twelve feet with a nine-foot ceiling and are large enough for Merlins to fly in circles. The ceiling is covered with polyethylene to protect the flight feathers from abrasion. Walls are approximately half concrete block and half flattened logs. Nesting facilities have included opportunities to build scrapes in turf placed on the floor, in a roofed ledge and in nest boxes made from wooden crates. The latter are 9½ x 14½ x 19½ inches in inside dimensions. A round, five-inch hole was provided with a perch beneath the hole in these boxes. In all cases, the material provided for scrape-building was the local forest floor which is largely sphagnum moss.

The Peregrines were housed in a separate electrically heated building in rooms with the same floor dimensions as the Merlin rooms, but with a roof sloping upward from five and one-half feet to about nine feet. The interiors of the Peregrine rooms are insulated with white, rigid insulation. Nest ledges and open-ended boxes about two feet on a side and two and one-half feet in the longest dimension were provided and have been filled with forest floor or pea gravel.

All rooms were equipped in 1969 with boxes to house a television camera which is moved from place to place as needed. Signal propagation is by radio transmission although closed circuit is feasible. The usefulness of television is hard to overrate when you wish to keep close track of the birds. All rooms are also provided with an intercom sound monitor. Reception of both sound and television is normally arranged for in my living room where nearly constant monitoring is possible.

Incandescent bulbs were used in two ceiling fixtures in each room. Normally 150-watt bulbs were used when simulating long days; 60-watt bulbs were used for short days. Timers were used for all lights, but no dimmers were utilized. A seven-watt night light was always used with the Merlins, but not in all cases with the Peregrines. Most changes of photoperiod were made abruptly and in almost all cases increases were made equally at morning and evening so that noon, standard time, was the point of symmetry. All rooms were "mapped" with a light meter aimed toward the nearest light bulb and attention was paid to keeping light intensities close to the same for both Merlins and Peregrines. Details are not reported here, but at "long-day intensities" the perches commonly used ranged from 13 to 41 foot-candles in both species.

### *Feeding*

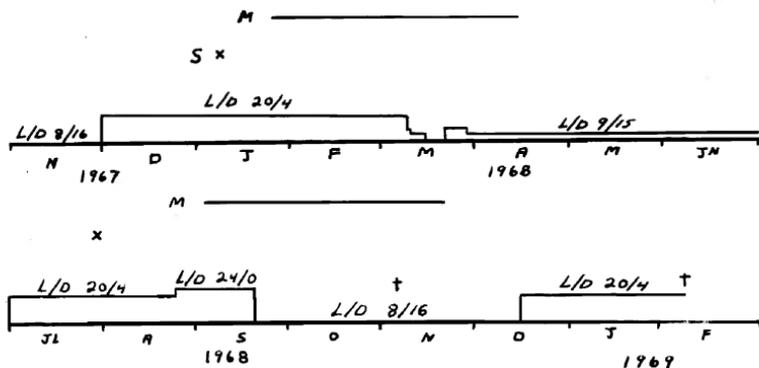
Over the years feeding has varied somewhat. The Merlins have always been fed twice a day and one feeding has always included a mouse. A laboratory mouse colony has been maintained and for the past two years the mice have commonly been fed carrots during the four days preceding killing and freezing. This treatment increases the carotenoid intake of the falcons and keeps the feet and cere yellow and, hopefully, provides for yellow egg yolks. These mice were also fed to the Peregrines at irregular intervals. During the last year large numbers of chickens were raised for the Peregrines. Heavy muscle and store bought chicken have been used, but all commercial food has been carefully trimmed of fat so as to provide for minimum intake of pesticides and PCB. While the outdoor Peregrine quarters are heated, winter temperatures in Fairbanks often cause temperatures inside the quarters to drop below freezing, sometimes to about 10 F. Feedings of the Peregrines are increased to twice a day at such times; at less severe temperatures only one meal is provided. Vitamin and mineral supplements are used at times, principally "Vionate." Special attention is paid to diet quality and quantity during long-day periods when breeding might be expected. Food to excess, often twice a day, is provided at such times.

### *Results and Discussion: Merlins*

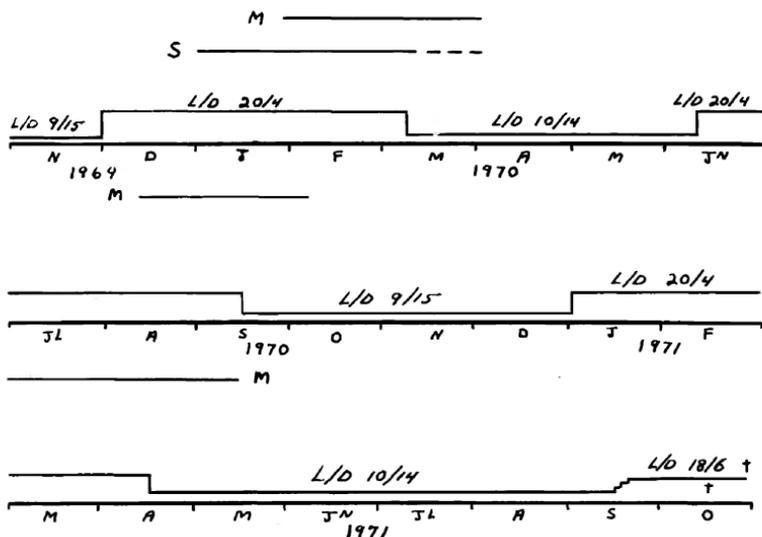
Basic data and experimental conditions are presented in Figure 1. Data are frequently ambiguous in endeavors of this sort and the figures record those events which can be clearly and definitely established, such as scrape-building and the beginning and end of a molt. Except in the first and last Merlin cycle, molted flight feathers were picked up and labeled with the date. Detailed data on sequence of feathers molted in all birds are available but are not presented here. Inception of new calls and other features of breeding behavior were noted but are not considered in detail in this paper.

Perhaps the single most striking feature of the Merlin data is the reproducibility from cycle to cycle with the same "pair" and from "pair to pair." Since light intensity has not been the same in all cycles, it is apparent that intensities

**Additional notes on Figure 1.** The lines marked with, for example, L D 8/16, represent the photoperiodic regime, and means eight hours of light/16 hours of dark. In the Merlin experiments, it was not always possible to determine whether or not a scrape had been built nor to determine the state of a scrape at all times without undue disturbance. The line or "x" marked S indicates the day, when known, when a scrape was begun and the duration of active maintenance, when known. In all cases a scrape was built as verified by inspection at the end of long days. The line marked M indicates the period of molt and does not distinguish between individuals but indicates the entire period when either bird was dropping primaries or tail feathers. The symbol † indicates death of an experimental bird.



TWO FEMALE MERLINS



MALE AND FEMALE MERLINS

Figure 1. Merlin experiments. Top: these represent the experiments with two females; Bottom: a pair. See additional notes on opposite page.

must be well above threshold levels during long days to be effective in inducing breeding behavior and molt.

It is also clear that some social factors which might be expected to affect timing do not do so. One "pair" consisted of two females and the lack of a male would seem likely to be very important if there are behavioral factors acting in concert with physiological factors to bring about such events as scrape-building and molt. At least in this single case there is a clear implication that behavioral considerations (apart from such survival issues as thoroughly incompatible pairs) may be a relatively minor consideration to the breeder, at least until the "hurdle" of the final burst of vitellogenesis in the female is reached. From here on it is likely that appropriate behavior on the part of the male is important for the female to enter the final phase of follicle growth, but again it seems likely that the appropriate behavior by the male is quite firmly determined by physiological factors, specifically by androgen levels.

The first duo of females produced some interesting behavioral data. Both size and personality of these females differed considerably. The smaller weighed about 200 g and was the more aggressive. She was the dominant bird except during one short period. The other varied between about 225 and 230 g and during her whole life had a slightly impaired function of her left wing. Under short day conditions the dominant, smaller bird always came down to the food first. When long days had persisted long enough to bring the birds close to the threshold of "courtship feeding," the dominant bird ordinarily allowed the other to go down first. On one occasion I observed the dominant bird allowing the other to pick tidbits from her beak in a session lasting several minutes. This may have been an abortive form of courtship feeding. During a later cycle the "submissive" female apparently reached a more advanced point in her hormonal development than she had in previous cycles, and conflict eventually developed over possession of the nest box.

During the course of the experiments with the pair, the length of the short day period was increased with each cycle in order to test the possible influence of a refractory period. At least for breeding behavior up to scrape-building and through molt, it appears that the refractory period is less than 81 days.

One of the initial assumptions—time as such is not important, but the completion of biochemical sequences is—implies that birds might be brought to sexual maturity at a point in real time earlier than normal for birds in the wild. Since successful reproduction was not obtained in these experiments, the issue remains undecided. It is a point to be kept in mind, however. The ease with which the Merlins were induced to complete about two cycles per year with respect to breeding behavior and molt raises some interesting questions. For example, with a number of species acceleration of "aging" may be possible. More than one "brood" might be brought off within a single year. It should be clearly noted, however, that such acceleration was less evident with the Peregrines. In the larger falcons, in which the issues of earlier maturity and more frequent breeding seem more urgent at the present time, acceleration may not be possible.

The failure of these experiments to produce actual breeding was something

of a surprise to me and obviously one is obsessed by the question of why they did not breed. There are few facts to support any hypothesis, but there are suggestions in the data. In the case of the two female Merlins, I performed laparotomies 17 and 31 days after the inception of molt in their second 1968 cycle. (I was unable to determine the exact date of first feather drop individually on these birds.) The follicles in each case were minute. Possibly the minute follicles seen in the first of these laparotomies (see Risser 1972 for a technique for laparotomies similar to the one I used) imply that the ovarian follicles never approached the final stage of vitellogenesis. In the fourth and final cycle of the male and female there are some better indications. On the 29th day after day length was increased, the male was found dead. The condition of the bird was excellent and the apparent cause of death, judging from blood in the mouth and beneath the skin of the neck, was trauma possibly traceable to the female chasing him around the room. To this point breeding behavior had been essentially as it had been in previous cycles. The testes measured 5.0 x 4.0 mm (L) and 4.7 x 3.6 mm (R) and were clearly above minimal levels. On the 41st day the female died about 16 hours following an experimental injection of diethyl stilbestrol (5 mg), FSH (17 mg Armour Standard Porcine) and LH (8 mg Armour Standard Porcine) into the pectoral muscles for the purpose of promoting follicle maturation and ovulation. A glycerol repository was used (0.7 ml). Death may have been caused by poor needle placement resulting in direct entry of the injection into the blood stream. Examination disclosed a full clutch of five eggs "on the way" and the five largest follicles measured 14, 9.2, 4.9, 4.2 and 3.4 mm. The extended oviduct measured 28 cm in length and weighed 5.8 g. The epipubic bones were fully spread and the vent was relaxed and engorged. Two large but atretic follicles were present. These data present several interpretive dilemmas. There is no way of knowing whether or not she was actually going to lay in the absence of the male. It is possible that many failures to lay are accompanied by follicle growth such as that seen here, but atresia of all of them occurs and the investigator never knows. The earlier of the two laparotomies on the member of the female duo does not shed definitive light on the point, but it could be interpreted as implying that birds which do not lay do not typically go so far in follicle growth. This situation would be more consistent with what is known about passerines.

The atretic follicles pose a further problem. Is it normal for some follicles to become atretic, thereby playing a role in the regulation of clutch size? There were, in fact, five follicles in healthy condition and five is almost invariably the clutch size of Merlins in interior Alaska.

The male died twelve days before the measurements of the female's follicles were taken. This fact gives us some firm information. It is evident that the female was in the final phases of rapid vitellogenesis. It is also quite clear that if the male's behavior or mere presence with the female was the stimulus which, via the central nervous system, was the factor which caused her to enter this final phase, his role had already been played twelve days earlier. This would be at the 29th long day and, judging from the size of the follicles, about 17 days before the first egg was due to be laid. It is interesting and perhaps significant

to note that he was frequently chased by the female. No courtship feeding or other evidence of a "good" sexual relationship with the female was observed before his death. He did, however, visit the nest box and show other behavioral evidence that he was feeling the impact of increased levels of reproductive hormones.

Although again it should be emphasized that one ought not make too much of such data, there were some experimental conditions present in this last, perhaps nearly successful, cycle that were not previously present. These conditions indicate something about procedures to be followed in the future. Previously, I had entered the room to change bath water and even when feeding I was often obvious to the birds when opening the door to toss in food. Just before long days were instituted in the last cycle I had prepared the room with a dark, cloth curtain perforated for a field of view for the television camera. This allowed me to complete all feeding and maintenance functions without being seen. No light bulbs burned out during this experiment so entrance to replace bulbs had not been necessary. Furthermore, this cycle had been preceded with a five-month short day period. It is possible that (1) I had been rushing the refractory period previously or (2) I was overly disturbing the birds previously, although there are no other data to suggest either of these.

Another factor which may have had something to do with the apparent near success of the last cycle was the fact that for the first time the long-day period was not instituted quite so abruptly as previously. Increases were staged as follows: day number 1, L/D 10/14 up to 12/12; day number 3, up to L/D 13/11; day number 4, up to L/D 14/10; day number 5, up to L/D 15/9; day number 6, up to L/D 16/8; day number 13, L/D 17/7 and day number 14 L/D 18/6. This was neither a smooth nor a "natural" increase, but it nonetheless resembled the normal wild condition more than the abrupt increases used previously. The final day length was also different from any previously used being, in fact, two hours shorter. This corresponds fairly closely with the natural day length experienced by the parents of these birds when they began their clutch in 1969.

While the implications that molt data may have on breeding condition are debatable, breeding and molt are at least related functionally and deserve consideration. Molts in my experience were rapid, in some cases so rapid as to leave the birds almost flightless. Extremely rapid molts were confined to the two females, although the pair molted more rapidly than do wild birds. In the case of the pair, female molts lasted (from first primary dropped to last primary dropped) 47, 53 and 56 days ( $\bar{X}$  52 days); molts of the males lasted 55, 50 and 69 days ( $\bar{X}$  58 days). All tail feathers were molted within this period; secondaries were molted slightly later, but are not considered in detail here. All molts were complete even though long days were terminated at different points in the cycle. On two occasions male and female molt began on the same day and on one occasion the male began three days later. Rapid molts may in some cases be related to abrupt shortening of day length. Cade (pers. comm.) feels that kestrel molts may have been speeded up in some of his work by sudden imposition of short days. Little evidence suggests that this is the case here. What rapid molts may imply here is that the photoperiodic regime was sufficiently abnormal to

the birds that at least some of the endocrine machinery, possibly portions vital to breeding, was not operating normally. It seems possible that the rate of increase and decrease, the length of the light period, and possibly the intensity may all be involved. It is interesting to note, however, that Willoughby and Cade (1964) in their successful work with kestrels from more southern areas used similar conditions.

Detailed analysis of molt sequence and molt performance of the different sexes will not be made here. It is remarkable, however, how consistent molt was between different birds and different cycles. On four of the five molts occurring during the seven cycles reported here, the first feather was dropped (female) on the 58th day after long days were instituted. The single departure from this schedule could not be accurately determined but the first feather dropped on about the 65th day. The female duo later died, apparently from a prolonged carbon monoxide episode in the house. They were doubtless experiencing some intoxication during this cycle which may well have influenced the molt. This astonishing congruence of the start of molts strengthens the case that the results were indeed induced by photoperiodic manipulation.

#### *Results and Discussion: Peregrines*

Figures 2 and 3 present the Peregrine data. The Peregrine results are more difficult to deal with than are those from the Merlins. Not only do they suffer from the same problems of small sample size and lack of controls, but in extrapolating from Merlin data on speed of response I was led into an experimental schedule which may have been rushing the refractory period for Peregrines. It is also evident that Peregrines may be quite different from Merlins not only in speed of response but, perhaps, in a requirement for greater complexity in photoperiodic manipulation, as well. I have had, as yet, too little time to adjust my experimental procedures to my growing awareness of the problems. Unfortunately, the issue of sexual maturity is prominent in the Peregrine work. The only bird definitely old enough to be expected to breed was a female which has never had a male. The pair will not be three years old until the summer of 1972.

I will proceed, however, to discuss the results analytically, to some degree at least, because there are some interesting hints of important questions to be considered in future years.

The first experimental photoperiodic regime was imposed on all birds during the winter of 1969-70. This was the third winter for the lone female and the first for the young pair. Long days were begun on all of them on December 1, 1969, and 22 days later the lone female began a scrape which she enthusiastically maintained for about a week. During this same period, she molted 12 flight feathers which completed her previous molt. After this brief episode a relatively quiescent period of about 90 days followed and then a scrape was built which was maintained for almost five months, four months of which were after the lights had been shut down to L/D 10/14. The response of the young birds was similar, but out of phase by about a month and no scrape was built.

With the Merlins it was typical that all things happened with clarity and dis-

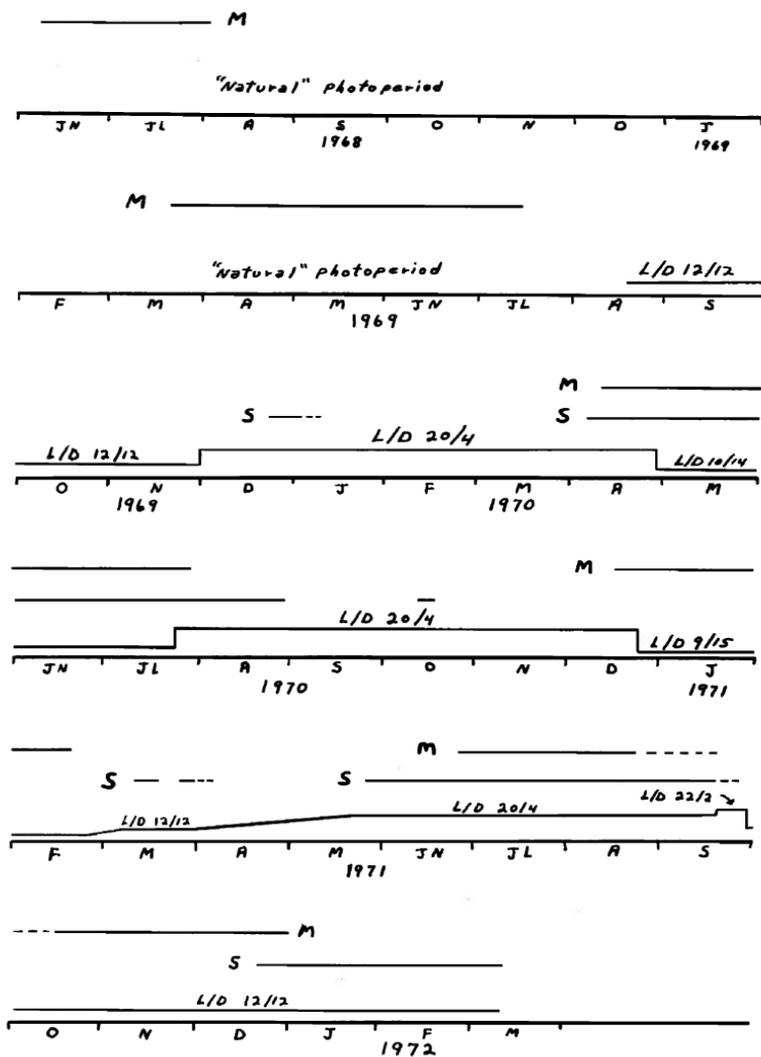


Figure 2. Lone female Peregrine. Symbols as in Figure 1.

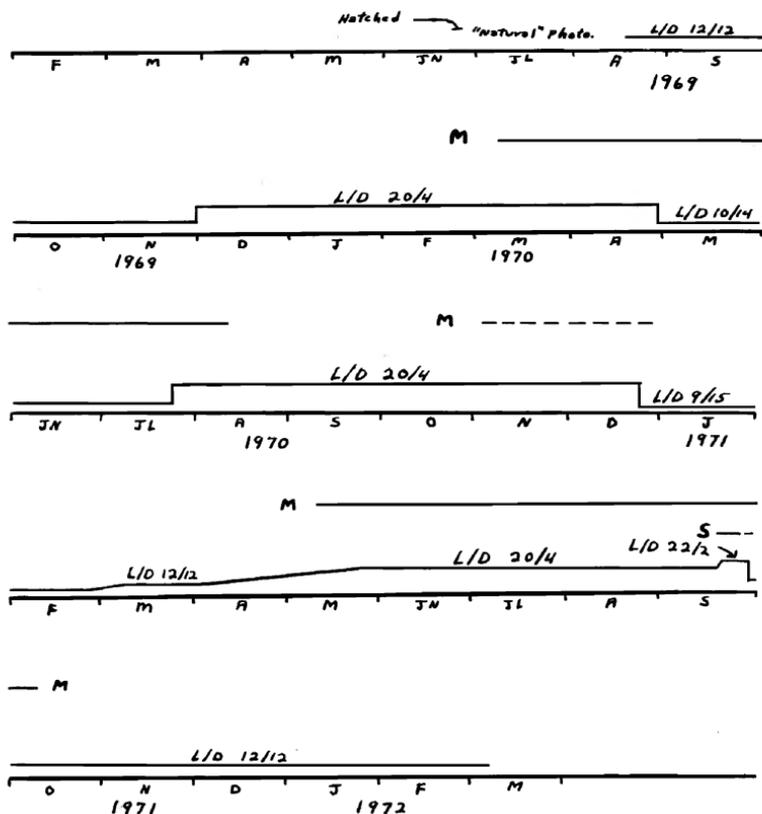


Figure 3. Male and Female Peregrines. Symbols as in Figure 1.

patch. Northern Peregrines seem more indolent in their response to photoperiod and there are several occurrences, particularly in the case of the lone female, which cannot be rationalized at this time. For example, although long days were instituted on December 1, 1969, "real" scrape building and molt did not occur until April in the case of the lone female. This is only about a month and one-half before she would have been expected to molt in the wild. She herself came from an egg laid on about May 21. Molt began in March in the case of the young birds, perhaps a bit over two months early. While this does argue for photoperiod having an effect, it suggests that I was unsuccessfully attempting to hurry a natural rhythm.

In the second experiment, I attempted to complete another cycle before the Fairbanks winter could exert too serious an effect, and began long days before molt was complete (although it was nearly so in the young birds) and before the older female had abandoned her scrape maintenance. After slightly over a month her scrape maintenance ceased. No further response was seen for an extended period and, when it did come, both scrape building in the lone female and molt in the young pair seemed somewhat tentative. Again, in the lone female, a brief period of scrape building occurred well after completion of activities of the previous cycle and well before she began scrape building in greater earnest. Her scrape building was not well developed during December, January and February, and at that time I attributed this lack of enthusiasm to the cold. More recent evidence suggests that this was probably not the inhibiting factor, since she has been very active and attentive with her scrape during the present December, January, February and March (1971 and 1972).

The final cycle reported here was an attempt to mimic their natural photoperiodic experience. It was also timed to come to fruition during the normal part of the year for the wild birds. Scrape building and molt in the lone female began approximately on time suggesting that the short day period begun in late December may have been a sufficient allowance for a refractory period. The young birds were closer to normal timing in the inception of molt than the lone female. In this case, perhaps suggesting that the young birds were simply not yet old enough until that point, a scrape was finally built in late September. For about a month, including the short period of scrape building, both young birds were "ee-chupping."

Recently, a number of people have commented on fall sexual activity in captive Peregrines, even to the point of egg-laying (Galicz *et al.* 1971). Nelson (this issue) has put this observation in perspective, pointing out that fall reproductive activity is quite common among birds. The hypotheses advanced to explain this widespread phenomenon on photoperiodic and endocrinological grounds are so far unsatisfying (see Farner 1959). My Peregrine data do nothing to explain the phenomenon but are, at least, interesting. The old, lone female has shown what might be taken to represent "fall" activity on at least three occasions. At the present time (March 10) she is enthusiastically maintaining and spending a great deal of time sitting on a scrape she began almost three months ago. She began this activity after more than two and one-half months on short days (L D 12/12) and she is still on short days. This is confusing, since it seems not to have the tentative character of previous, possible analogous, occasions of this sort. The young pair has shown an increase (the female, at least) in "ee-chupping" at feeding time during the past month (mid-February to mid-March), but otherwise they have shown no increased interest in reproduction. I do not know what these short bursts of activity imply; they do not even have the consistency of occurring on either long or short days. It does seem possible that L D 12/12 is sufficiently short to allow photorefractoriness to be dissipated, yet long enough to be photostimulating when the refractory period is past.

There are some hints that the birds may not really be responding to photoperiod with respect to this phenomenon and, in fact, an all-over look at the data might suggest that there is an implication of a yearly endogenous rhythm, a "circannian" rhythm (see Marshall 1961, and Immelmann 1971 for further discussion of this point).

There are many interesting attributes of the Peregrine data, but throughout one must realize that the data simply are too confused, fragmentary and lacking in both controls and sample size to support even the most enthusiastic speculation. I am, however, left with the distinct impression that for now we ought not depart too much from "natural" conditions with northern Peregrines. Further, there seems ample reason to assume as a working hypothesis that circannian rhythms (and diurnal rhythms) might be important, and to extend the hypothesis (perhaps beyond its stretching capacity) that one can "set" diurnal or annual "clocks" without intending to, by photoperiodic manipulations. There is at least some evidence that this might occur in the statements of a few falconers that eyass "tundra" birds taken to mid-latitudes at an early age molt in a manner consistent with "anatum" birds of the same latitude.

The question of why the Peregrines did not lay eggs may be gratuitous, since only the lone female was over two years old at the end of the work reported here. Unfortunately, we are quite in the dark about a crucial issue—the age required for sexual maturity in Peregrines. There are field observations of brown females attending eggs, although the original "layer" of such eggs is usually in doubt. The fact that typical adult plumage is normally attained before the bird is two years old is puzzling, if the bird is not to become a member of the breeding population. Nelson (1971) notes Peale's Peregrines which almost certainly bred at two years of age. In 1970 I found a molted brown primary (Right, number 8) beneath a Yukon River eyrie which fledged young. Obviously, the wild situation is different from the captive situation and although it seems probable that two year old birds *can* reproduce, we must wait for firm data.

In my own experiments the older female was certainly old enough to breed. The most obvious reason for her not laying eggs, if the experimental regime was otherwise satisfactory, may lie in the absence of a male. During the third cycle for the young birds it does seem possible that they were old enough, but I know of no northern Peregrines which have laid eggs in captivity at that age. The coming season will be interesting with regard to any "aging" effect due to photoperiodic cycles faster than normal, since they will still, by the clock, be rather young for egg-laying in captivity. Richard A. Graham (pers. comm.) has commented on the strong tendency for paired, captive female Peregrines of four years of age to lay eggs and for younger birds not to lay, although no taiga or tundra Peregrines are involved in this generality.

### *Comparisons and Generalities*

While the experimental design for neither species provides for a rigorous test of photoperiodism, the data really allow no other interpretation. The case is particularly strong for Merlins, although the Peregrines also manifested evidence

of response in both breeding behavior and molt outside of the normal wild breeding season.

Merlins respond rapidly and unambiguously. With the possible exception of the last cycle reported, there are hints that the experimental conditions have not been optimal for actual breeding. Molts, for example, were complete, apparently to the last body feather, but the rapidity suggests a fundamental abnormality, possibly reflecting unnaturally high levels of thyrotrophic hormone. Whether or not this hypothesized hyperactivity on the part of the anterior pituitary extends to the cells responsible for secretion of gonadotrophins is a moot point. If gonadotrophin secretion is too rapid or too high, it could certainly contribute to failure of breeding. While indulging in somewhat tortuous reasoning of this kind, it should also be pointed out that the results in some respects imply too *low* levels of gonadotrophins, but do not have anything to say on the issue of speed of secretion. Quite obviously we need detailed information on circulating levels of the hormones involved in reproduction to answer such questions.

There are many ambiguities in the Peregrine data which do not yet permit even hypotheses to be formed with much sense of security. It does seem clear that under comparable photoperiodic regimes Peregrines are slower to respond. Possibly these differences in the experimental response are correlated with differences in migration patterns. It is at least equally possible that the differences are due to experimental design. It is interesting that some molts in Peregrines have tended to be relatively close to normal wild molts in duration and in sequence. This may imply that experimental conditions were closer to those needed for breeding than were those used for the Merlins. In my opinion, the great behavioral responsiveness of the lone female Peregrine argues that she has been relatively close to egg-laying. In many ways she acts toward me as she should toward a male Peregrine, even to the extent of copulatory movements when I stroke her back or tickle her belly and cloaca. I have attempted to bring her along further by playing the part of the male without success.

One lesson seems clearly underlined by these studies, faulted as they are at this early stage: Even though examination of two species may suggest that they should respond similarly to photoperiod, judging from breeding locality, phylogenetic relationship, migration patterns and other generalities of the ecological setting of the species, we cannot assume that they actually do have the same photoperiodic requirements. Extrapolations and comparisons will be very valuable, but such must be made with caution.

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(Manuscript received March 15, 1972.)

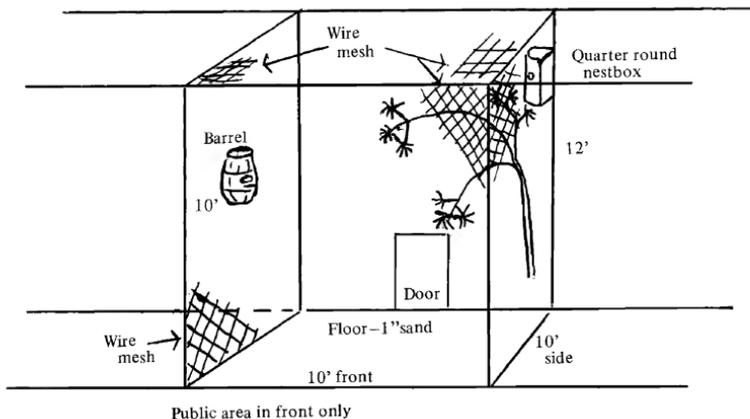
REPORTS, REVIEWS, AND OPINION

REPORT: BREEDING PROJECT INFORMATION EXCHANGE

The numbers in the BPIE reports correspond to the questionnaire printed in *Raptor Research* 6(1):16-18.

B.P.I.E. No. 41. American Kestrel.

1. a. Walter English, Don Beery. b. Woodland Park Zoo, Seattle, Washington. c. May be used in *Raptor Research*.
2. Date of report: January 21, 1972 for period of 1968-1971.
3. Species: American Kestrel, *Falco sparverius*.
4. Age: male unknown, female number 1 unknown, female number 2, hatched spring of 1970.
5. Origin: male haggard, female number 1 haggard, female number 2 eyass.
6. Eyrie: area Puget Sound.
7. Handling: male and female number 1 manned and flown free, female number 2 manned but not flown.
8. See sketch; floor concrete, with one inch of sand; top screen with fiberglass panels two feet above.
9. No artificial light.
10. Color: back wall grey sheetrock, sides and front wire screen top, white fiberglass panels.



11. Provisions made for nest: two nests supplied, small barrel and quarter round box; birds chose quarter round box located in back of cage with pine branch between nest and front of cage.

12. Introduction to facility and to each other: male and female number 1 were kept on screen perch together for one year and put in cage together January 1968, female number 2 was put in cage after loss of female number 1, October 1970.

13. Behavior to each other: male and female number 1 very compatible (noticed mutual feeding); male and female number 2, male was aggressive for short period (one day) then appeared not to notice her for one week, accepted her after that.

14. Nest building: scrape was observed in nest, not known which bird made scrape.

15. Food: two adult laboratory mice per bird a day and live crickets at least once a week.

16. —

17. Was copulation observed: yes, date and time of day not recorded, male mounting female on branch provided for perch.

18. —

19. Eggs: one egg 1968, two eggs 1969, no eggs 1970, five eggs 1971.

20. When did incubation start: no records on exact incubation dates. Did both birds incubate: no, male would chase female back onto nest.

21-23. —

24. If eggs did not hatch, were they fertile: 1971 eggs were fertile but did not hatch, embryo was 18 days old, unknown on 1968 egg, was stolen from nest after 11 days.

25. If parents did hatch eggs: dates, May 19 and May 21, 1969; behavior of parents, female became very aggressive toward people.

26-27. —

28. Food: mice, chicks and sparrows. Feeding of young: both parents fed young.

29. —

30. How long were young with parents: 10 months. Parent offspring behavior: only significant behavior noted, after four months young became aggressive to parents and would chase them from food tray.

31. —

32. Final description of F1 generation: put into another breeding project. No information as yet.

33. Additional information: there was no laying in 1970. Two possibilities— young from 1969 were left in with parent birds too long, and a passage Swainson's was placed in cage next to kestrels, in February male kestrel was observed flying against wire and screaming at Swainson's, which was removed around the first of March. This project since its beginning has not been isolated, and has thousands of Zoo visitors going by every month without any effect on the breeding cycle of these birds, except in 1970 someone cut the wire on the front of the cage and took the breeding female.

## NOTES, NEWS, AND QUERIES

**Editorial Notes.** Interesting material has been arriving and is being processed for the next two issues; we look forward to the receipt of other contributions. Two parts of the Supplement are published with the current number, four others are in process, and six others will be done as soon as possible. The first issue of *Raptor Research Abstracts* is in process and we hope to be caught up on both series in December.

**Publication Date of Spring Issue 1972.** Volume 6, Number 1, Spring 1972 was officially published on September 23, 1972.

**Cover Illustration.** Our new cover illustration is the art work of Robert Katona of Denver, Colorado. We are grateful to Mr. Katona for this contribution. It is based on a photograph of a Prairie Falcon taken by Babette Cranson. We will have further information in a future issue.

**Radio Tagging Raptors?** The Biotelemetry Committee of the Raptor Research Foundation, Inc. is conducting a survey in an effort to compile and make available information about recent studies utilizing biotelemetry in raptor research studies. If you are using telemetry techniques in raptor research work and would like to be included in the survey, please send your name and address to: Mark Roy Fuller, University of Minnesota, Department of Ecology and Behavioral Biology, Bell Museum of Natural History, Minneapolis, MN 55455. A questionnaire will be sent to you. A summary of the information obtained in response to these questionnaires will be sent to all respondents. Thank you for your cooperation.

**British Peregrines—The 1971 Situation.** The census of the Great Britain Peregrine breeding population in 1971 involved visits to 689 out of 806 known territories where breeding has been attempted since 1930, giving 86% coverage. (All percentages are based on number of territories visited.) Out of the 689,

319 (46%) of territories were occupied by Peregrines, either pairs or single birds, and in 142 (20%) of these, broods of large young were found. Probably most of these broods successfully flew, and only five additional broods were known to have been removed. Occupation of territories and successful breeding showed the same general increase from south to north which characterized the Peregrine population during the previous enquiry of 1961-1962. In 1971 only 32 occupied territories (13%) were known in southern England and Wales combined, and only eight broods (3%) were reared. In northern England and southern Scotland combined, there were 67 occupied territories (54%) and 21 broods (17%) were reared. The bulk of the population is still concentrated in the Scottish Highlands and Islands, and here there were 220 occupied territories (67%) and 113 successful broods (35%).

The British Peregrine population appeared to reach its lowest ebb in 1963, when a sample census gave 38% occupation of territories, and 10% of territories producing flying young. Up to 1966 there was little change, but a slow increase in numbers and breeding success has since become apparent. This recovery has been mainly in districts north of the Crown Pennines. Moreover, the total figures conceal the finding that there has been a substantial improvement in inland northern districts, but virtually no change on the coast. Nationally, occupation of territories and breeding success for inland districts were more than double those for coastal districts.

The post-1955 Peregrine decline has been attributed largely to widespread contamination of the species by persistent, toxic pesticide residues, and it was predicted that a decrease in use of these chemicals should be followed by an improvement in status of the Peregrine, first in the least contaminated northern districts. Analysis of Peregrine eggs from northern inland districts confirms that recovery here has been paralleled by a decrease in organochlorine insecticide residues since 1966. The absence of recovery in northern coastal districts may be connected with general predation on sea birds by these Peregrines. Available evidence suggests that the other pollutants (especially the PCBs), to which these coastal birds are exposed through their marine food chains, persist at a relatively high level in the sea, and may well have increased during the last decade.

Successful broods averaged 2.1 young nationally, so that about 300 young Peregrines were reared in Britain in 1971. A similar output has been estimated for 1970. With this order of annual recruitment, there would seem to be enough surplus Peregrines to allow fairly rapid recovery in the still depleted districts of Wales and southern England. Adverse factors must therefore be operating against recovery here and though the simplest explanation is that level of exposure to toxic chemical residues may still be critically high in southern Britain the reasons could be complex.

A final, detailed report on the Enquiry is being prepared for publication in *Bird Study*. The figures given above for 1971 are not final, but will probably not change significantly. (By D. A. Ratcliffe; from *BTO News* 49:1, February 1972.)

**Rare Captive Falcon Hatch Noted.** Three new falcon chicks have been bred by a New York state ornithologist who only last year succeeded in breeding the first falcon in captivity. Dr. Heinz Meng, a professor of biology at the State University of New York at New Paltz, raised conservationists' hopes for consistent breeding of the birds in captivity when the three Peregrine Falcon chicks were hatched this spring. As this CN issue goes to press, Dr. Meng is awaiting the birth of a second clutch nearly ready to hatch. Until the first chick was hatched last year, it was considered nearly impossible to breed hawks and eagles, high on the list of endangered species, in captivity and thus save them from extinction.

Breeding of the birds has proved difficult in the past because their mating behavior pattern includes a spectacular, soaring, aerobatic flight that is not possible in a cage. Dr. Meng said that after seven unsuccessful years of trying to breed the birds, he finally succeeded by "thinking like a bird," constructing a natural hawk environment in his own backyard, complete with sun shelves, padded perches, and a ledge that looked like a rock cliff. (From *Conservation News* 37(10):10, July 1, 1972.)

**Endangered Species Legislation to Be Taken up Soon.** Both the House Committee on Merchant Marine and Fisheries and the Senate Commerce Committee are scheduled to begin marking up legislation which will further protect the nation's threatened wildlife species. The pending legislation includes two House bills (H.R. 13081 and H.R. 13111) and three Senate bills (S. 249, S. 3199, and S. 3818).

The proposed legislation is surrounded by some controversy, due to the fact that all of the bills seem to ignore opportunities to draw on existing scientific, law enforcement, and other expertise already available in federal and state wildlife agencies. The present legislation would vest full authority for all designated endangered species, at home and abroad, in the Bureau of Sport Fisheries and Wildlife. The Bureau, unfortunately, is one of the most inadequately staffed and poorly financed agencies of the Department of the Interior.

Most conservationists feel that in order for endangered species legislation to work a strong and closely coordinated federal-state program is needed. Federal authority is needed to handle international and interstate problems, while state participation is needed to respond to mainly intrastate problems. And the combined scientific expertise and law enforcement manpower and financial resources of both levels of government are needed to mount the most effective program.

In a comparison, state wildlife agency personnel total more than 18,000 in the fifty states, while the Bureau is budgeted for about 3,960 employees nationwide. As for funding, the Bureau's net appropriation for this year is \$92.9 million while the states spent in excess of \$247 million last year. (From *Conservation Report* 92nd Congress, 2nd Session, Report 28:303, September 8, 1972.)

## **RAPTOR RESEARCH FOUNDATION, INC.**

in care of Biology Department  
University of South Dakota  
Vermillion, South Dakota 57069  
U.S.A.

The **RAPTOR RESEARCH FOUNDATION, INC.** is a non-profit corporation whose purpose is to stimulate, coordinate, direct, and conduct research in the biology and management of birds of prey, and to promote a better public understanding and appreciation of the value of these birds.

Publication has been a major area of activity. From 1967 to 1971 *Raptor Research News* was published; in 1972 publication was continued under a new name, *Raptor Research*. A series of occasional longer publications was started in 1971, *Raptor Research Report*. *Raptor Research Abstracts*, initiated in 1972, is a quarterly bibliographic service.

The Raptor Research Foundation has had a number of informal meetings and in 1971 sponsored the first of its conferences on specific topics on raptors. This one was entitled "Special Conference on Captivity Breeding of Raptors," and another planned for 1973 is entitled "Conference on Raptor Conservation Techniques."

The interests of the Foundation are indicated by the titles of its committees: Editorial, Captivity Breeding, Population, Banding, Bio-telemetry, Pathology, Pesticide, Ecology and Ethology, Systematics, Education and Conservation, Bibliography, International Coordination, and Finance and Investment.

## **MEMBERSHIP**

Membership in the Raptor Research Foundation is open to all who contribute. *Raptor Research* is sent to all who contribute a minimum of \$3.00 per year; those who wish to receive both *Raptor Research* and *Raptor Research Abstracts* must contribute a minimum of \$5.00. These minimal rates have been established to encourage all who are interested to join. Other activities are financed by the generosity of members who contribute more than the minimum. Such contributions are encouraged.

## **PUBLICATIONS**

All previous publications are still available.

*Raptor Research News* each issue 50 cents.

1967-1969, Vols. 1-3, 4 issues each; Analytical Index, Vols. 1-3— 50 cents.

1970-1971, Vols. 4-5, 6 issues each; Vol. 5, issues 5-6 combined, \$1.00.

*Raptor Research Report No. 1*, Richard R. Olendorff, "Falconiform Reproduction; A Review. Part 1. The Pre-nestling Period." February 1971, 111 pp., 6" x 9", \$2.50 (\$2.00 to members).

Additional copies of current issues of *Raptor Research* are \$1 each. For price of additional copies of the Supplements, apply to Raptor Research Foundation.