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Communications

To the Editor:

I would like to say a few words about the California Condor and its plight. In 1978 U.S. Fish & Wildlife biologist, Sanford Wilbur, estimated a maximum population of 30 birds including six or seven in immature plumage in October, 1978. However, at a May 1979 meeting of the Northern California Chapter of the Cooper Ornithological Society, the late Dr. Carl Koford of the Museum of Vertebrate Zoology, University of California at Berkeley, indicated that the population may be as low as 20 birds — in fact, only 13 individuals were accounted for in the October, 1978 count. Dr. Koford then discussed the contingency recommendations of the Office of Endangered Species and presently supported in principle by the National Audubon Society. This plan includes (1) capturing *all* wild condors and attaching radio transmitters to their tail feathers, (2) holding five potential breeding pairs during the first two years, and later ten pairs, to initiate a captive breeding program, and (3) taking most eggs or chicks from *all* nests of wild condors! Dr. Koford then expressed grave doubts about the wisdom of such a program. First, the possibility of injury or death to newly captured birds is a real danger (nine of 14 South American captured Andean Condors died shortly after capture), and a certain degree of psychological trauma cannot be avoided. Second, the birds are to be captured on the Tejon Ranch, the current center of winter roosting and certainly the worst place to disturb them, especially considering the danger of taking adults with dependent fledglings or eggs. Third, this program would effectively remove virtually the entire wild California Condor population (including *all* immatures), thereby precluding any further study of the species in the wild. Fourth, the captive breeding program may or may not work. And fifth, even if it does work, just where on this planet can the species be effectively reintroduced? If the present day condor habitat cannot support a viable population of wild condors, how can anyone imagine that the habitat will improve in the future to the extent of supporting reintroduced cage-reared condors?! We

do not believe that forcing a population of captive condors to perpetuate themselves, through cage manipulations, amounts to "saving" this majestic bird. If you, like William Leon Dawson (*Birds of California*, 1923), are "not ashamed to have fallen in love with so gentle a ghoul," please make your views known to the National Audubon Society.

— Dave DeSante, Landbird Biologist
Point Reyes Bird Observatory
4990 Shoreline Highway
Stinson Beach, CA 94970

To the Editor:

Briefly, we are in complete agreement with Dr. DeSante and the late Dr. Koford that the California Condor is in deep trouble. We disagree, however, as to the best means by which to assist this highly endangered species, to halt its increasingly rapid decline toward extinction, and to ensure, so far as is humanly possible, the perpetuation of a wild, free-flying population of the condor — the ultimate goal of the entire program.

We, and I am here speaking for the National Audubon Society, believe that the California Condor is clearly facing extinction in the near future because it has failed to reproduce in the wild, for at least the past 15 years, at a rate sufficient to replace even the present population that is so greatly reduced in numbers and because, so far as we can judge on the basis of all available evidence, the population is declining at the rate of one to two individuals per year.

Audubon Conservation Report No. 6, *Report of the Advisory Panel on the California Condor* (1978), the work of an independent panel of nine distinguished scientists appointed jointly by the National Audubon Society and the American Ornithologists' Union, found a population estimate of 20 to 30 individuals, about one-quarter of which were immatures, to be reasonable. This is only half the size of the condor population that we were estimating only 10 years ago. Since 1965 reproduction of condors in the wild has averaged less than one young per year, and it appears that the future of the entire population may lie with only one or two breeding pairs. We

also have considerable evidence, which is discussed at length in the Report of the Advisory Panel, suggesting very heavy contamination of wild condors by DDE, PCBs and other persistent, long-lived pesticides, and Dr. Koford himself suggested the strong possibility that the condors may be contaminated by 1080 and other toxic chemicals used in their range. Similarly, much of the condor's historic foraging range is threatened by man's own activities in California.

All of these factors were considered by the Advisory Panel, and I would like here to quote from its Recommendations:

"Having reviewed the current status and prognosis for survival of the California Condor population, the Panel believes that the only hope for the species lies in a long-term, large-scale program involving greatly increased research effort, immediate steps to identify and conserve vast areas of suitable condor habitat, and captive propagation. The needs for research and habitat preservation are obvious. Opponents of captive propagation have argued that if these goals were met, the condor population would increase on its own, but for several reasons this point of view is vacuous. First, there is no evidence that present conditions in the existing condor range are conducive to increased survival or recruitment. The

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population may have to be relocated before it can sustain itself with minimum aid. Second, the population should be increased as rapidly as possible to avoid loss of genetic variability and to escape the danger of extinction. Captive propagation can increase the condor population far more rapidly than natural recruitment because survival in captivity is better assured, productivity can be enhanced greatly by artificial incubation and hand rearing of young, and pesticide contamination can be minimized."

Following publication of this report, the Advisory Panel's recommendations were subjected to an intensive in-house review by the U.S. Fish & Wildlife Service, which led to the approval by Director Lynn Greenwalt on February 23, 1979, of a document entitled "Recommendations for Implementing the California Condor Contingency Plan." That document spells out in explicit detail all of the steps and safeguards of a long-term program for the condor that in almost every respect meets the basic recommendations of the Advisory Panel. Subsequently, we testified in support of this program before both houses of the Congress and had perhaps the leading role in obtaining approval by the Congress of a substantial "add-on" appropriation to cover the ini-

tial, start-up costs of this program. At the same time the National Audubon Society committed itself to raising \$500,000 to cover our own costs as active participants in the research and public education aspects of the program over the next five years. To that end the Society has reassigned a senior member of our research staff, John C. Ogden, to full-time research on the condor for the next three to five years. Under the terms of a Cooperative Agreement between the National Audubon Society and the U.S. Department of the Interior, which will have been signed by the time this issue of *American Birds* is published, Ogden will work in close partnership with Dr. Noel F. R. Snyder, one of the most respected of the Service's endangered species biologists, in conducting the research program on the condor. Both Ogden and Snyder will commence their work in California in January 1980 and will be assisted in the field by several research technicians to be provided by the U.S. Forest Service, Bureau of Land Management, and California Department of Fish and Game.

Another of the Advisory Panel's recommendations was that the entire condor program be subjected to periodic review by an impartial panel of scientists; such review, again with the assistance of the American Ornithologists' Union, is provided for under the terms of the Cooperative Agreement mentioned above.

It should be understood that no one can guarantee that even this long-term program, which involves a 30- to 40-year commitment, will succeed in preserving the California Condor. As we stated in our testimony before the Congress, this program is a last-ditch effort and a gam-

ble, but it is an informed gamble, and at every stage in the program safeguards have been incorporated into the plan in order to minimize the chance of harm to wild condors and to maximize the likelihood of success of the procedures involved. But the National Audubon Society, the U.S. Fish and Wildlife Service, and now, by its endorsement of this long-term program, the Congress of the United States, are committed to the proposition that since man is clearly responsible for the condor's present plight, it is our responsibility and our duty to do all that can reasonably be done in an attempt to ensure the perpetuation of a wild, free-flying population of this magnificent species.

Finally, I would like to make one point that has occurred to all too few of those who have been engaged in the highly emotional debate over what must be done to give the California Condor the best possible chance for survival. Even if this long-term, large-scale condor rescue effort, admittedly a desperate, last-chance operation, fails and we do not succeed in preserving the condor in the wild, we will have given this effort our very best shot, and the information that we will have gained and the techniques that we will have perfected in the course of that attempt may well be useful, even vital, in helping to prevent the extinction of other endangered members of the North American avifauna.

One of the alternatives to this condor program is that of giving up and simply standing by and chronicling the condor's rush toward extinction. That is an alternative that the National Audubon Society will not and cannot accept.

— Richard L. Plunkett, Staff

Ecologist, National Audubon Society

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"BIRDING IS OUR ONLY BUSINESS"

To the editor

This letter is to comment on the article in the May 1979 issue of *American Birds* by Mueller, Berger and Allez entitled "The Identification of North American Accipiters" (*AB* 33:236-40, 1979). We feel that this article has little to do with *field* identification of North American Accipiters, has little to do with in-hand identification of North American Accipiters, and contains some incorrect or misleading information.

We feel qualified to make these comments because we have undoubtedly handled or counted as many Accipiters in North America as have the authors. In addition, we have both been independently teaching raptor field identification in the classroom and the field.

First, to clear up some of the errors. On page 239, the authors state that "Adult ♂ Sharp-shinneds have caps fully as dark as adult ♂ Cooper's." A check of in-hand photos and live and dead specimens of both species, both sexes in adult plumage, confirmed that the Cooper's' caps are always darker colored than the back, *with a line of high contrast*. On the other hand, caps on museum specimens of Sharp-shinneds, if darker, show no contrasting line unless they are mounted unnaturally with their face looking up. If the study skin is prepared with the head in a "natural" position, no contrasting line shows and the cap appears the same color as the back (see accompanying photo). We certainly noted no sex difference in the caps of museum specimens, photographs, or live adult Sharp-shinned or Cooper's hawks.

Next, from the attached photos, it is obvious that the head on a Cooper's Hawk is *proportionately larger* than the head of a Sharp-shinned Hawk. We disagree also with the following statements given in the article: "Tail shape is an aid to identification but is definitely not a field mark." What is a field mark but an aid to identification? We have been *successfully* using tail shape as *one* field mark to help to identify Accipiters. In fact, the photos accompanying the article show the difference in tail shape very well. The authors neglected to mention a very good field mark, the thickness of the white bar on the tip of the tail (much thicker in Cooper's).

We question whether weight is as good a measure as the authors imply. Some raptor species fluctuate in weight by as much as 50 per cent. But mostly we disagree with the comment that "the system of field marks and identification by elimination simply does not work with hawks, falcons and eagles." We have been successfully using just such a system with very good results if we can *see the hawk well*. We feel that much of the information in this article such as the size diagrams and tail drawings are useless for any kind of identification.

The authors went to great length to show the dimorphism and variation in weights and body lengths of the Accipiters, but turned around and represented variable and dimorphic species, such as the Common Crow, Common Grackle, etc., as a single measurement with no separation of sexes or variation within a sex represented. This makes size comparisons based on the information given very difficult.

To give credit where due, we heartily concur that there is no overlap in size in the Accipiters of North America (but Storer reported this fact back in 1966), and we agree with the comments on flight differences due to wing loading differences. The photos were very good and correctly identified.

In summary, we have carefully considered all the objections raised in this article. However, both of us will continue to teach hawk identification using a system of field marks and a method of elimination, and we feel that we will do it successfully.

—William S. Clark, Director, Raptor Information Center, National Wildlife Federation, Washington, D.C., and Peter J. Dunne, Naturalist Director, Cape May Bird Observatory, Cape May Pt., NJ



♀ Sharp-shinned Hawk (left), ♂ Cooper's Hawk (right); note the relatively more massive head of the Cooper's Hawk. Photos/W. S. Clark.



♀ Sharp-shinned Hawk (left), ♂ Cooper's Hawk (right); note the difference in shape of tail and the distinct white band (bar) across the tip of the tail of the Cooper's Hawk.



Adult ♀ (top) and adult ♂ (bottom) Sharp-shinned Hawks. Note the obvious size differential between the ♀ and ♂ as well as the lack of contrasting black cap in this species.

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To the editor.

In discussing Mariana Islands birds, Pratt *et al.* (*Am. Birds* 33:227-235, 1979) overlooked, understandably, my note on "War and the birds of Guam" (*Indiana Aud. Soc. Yearbook* 24:53-56, 1946). Since earlier observations are limited, the following comments (based partly on the 1946 note and partly on memory) may be of some value.

From June to December, 1945, I made regular (and largely independent) observations in the Tumon Bay area of Guam, and made occasional visits to other parts of the island. My impressions of abundance at that time are in general agreement with those of Baker, Marshall and Stophlet. I was optimistic about the future of all of the Guam species discussed by Pratt *et al.*, except the mallard and megapode (not found), the gallinule and reed-warbler (too local), and the mannikin, sparrow and drongo (not there yet). The ground dove, fruit dove, swiftlet, starling and crow were all observed routinely, and the starling appeared to be thriving in recently disturbed areas. Upon comparing my impressions of 1945 with the description of the 1976 situation provided by Pratt *et al.*, it would seem

that these five species have declined even more rapidly than their habitats have dwindled. The same ominous pattern is less apparent for the broadbill, fantail and white-eye, though it is clearly suggested in the 1976 to 1978 time interval. Both the rail and the kingfisher were widespread and regularly found in 1945, but I would have called neither "abundant"; perhaps part of their decline is semantic.

In view of the obscurity of my earlier note, I might add that I observed a single Bristle-thighed Curlew (with Whimbrels) at Ylig Bay in fall migration. This species is not included in Owen's 1977 list of Guam birds.

—P. D. Skaar

Dept. of Biology, Montana State U.
Bozeman, MT 59717

To the editor:

Regarding "the most controversial bird in California's birding history" (*Am. Birds*, May 1979, p. 262), should it never be decided what it was California birders will get what they deserve. (I feel safe in saying this being somewhat removed from the scene!). Since the Skylark, or whatever it was, was there for

over two months and everyone interested in it should have had ample time to see it, why wasn't it collected? It was evidently a classic case where photos were simply not enough to pin it down. Are California ornithologists too timid to do what their predecessors would not have hesitated to do for posterity? Are the legions of birders and their Bambi-ite rantings too much for the pros on the West Coast? I say this as an amateur and lister myself. Well, if it were an Oriental Skylark, it may never appear on the North American list and ornithology as a science has been done a disservice.

—Robert A. Duncan
614 Fairpoint Dr.
Gulf Breeze, Fla. 32561

To the editor:

I wish to point out an error in the September 1979 issue of *American Birds*. You credited my honorable mention photograph of the White-tailed Ptarmigan to "William Irvin," when in fact my last name is spelled Ervin.

While I greatly appreciate the time and expense you have put into the Photo Salon as an opportunity for bird photographers, I was nevertheless disappointed

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to see my name misspelled. I hope that you will see to it that a correction is printed in the next issue. Lest you think I only look for mistakes, may I congratulate you and your printer on the fine quality of the reproduction of my transparency!

—William Ervin
843 Marine St.
Boulder, CO 80302

Our sincere apologies to William Ervin.
—Ed.

ANNOUNCEMENTS

NAME CHANGE

The Northeastern Bird-Banding Association announces a change in the name of its journal from *Bird-Banding* to *Journal of Field Ornithology*. This change will be effective with the first issue published in 1980.

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at the California Academy of Sciences in San Francisco. This program, the fourth in a series of symposia conducted by the Center, will discuss the status of various species in Mexico and present an overview of the research and conservation work that is being done there.

Speakers for the day-long program include Alexander Sprunt, IV, Director of the National Audubon Society's Research Department; Eugene Knoder, Director of the Society's Western Environmental Science Program; Dr. Clyde Jones, Director of the U.S. Fish and Wildlife Service's Denver Wildlife Research Center; Dr. Laurence Binford,

Chairman of the Department of Ornithology and Mammalogy at the California Academy of Sciences; Dr. Daniel Anderson, Professor of Wildlife and Fisheries Biology at the University of California at Davis; and representatives of the government of Mexico and the National University of Mexico.

Admission to the symposium will be by *advance registration only*. For tickets, send a self-addressed, stamped envelope and your check, made payable to the National Audubon Society, for \$4.50 per person, to **BIRDS**, Western Education Center, 376 Greenwood Beach Road, Tiburon, CA 94920.

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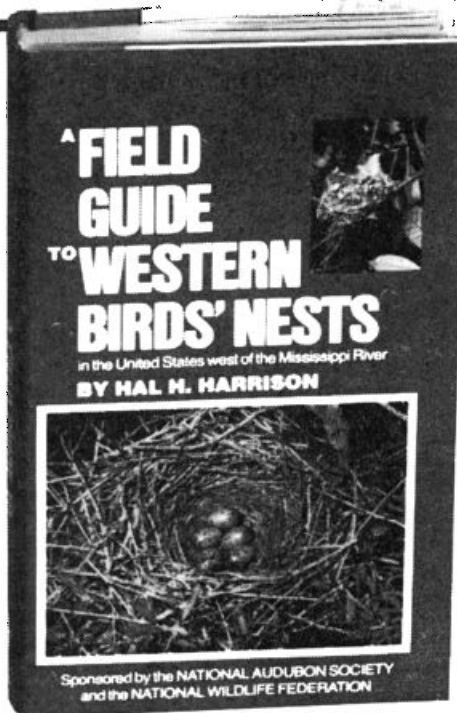
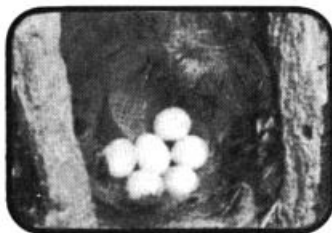


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