*The Condor* 87:446


# COMMENTARY

## CALIFORNIA CONDOR POPULATION ESTIMATES

I question certain statements that appear in the lead article of the February 1985 issue of *The Condor,* under the title, "Photographic censusing of the 1982–1983 California Condor population," authored by Noel F. R. Snyder and Eric V. Johnson. On page 11, under the heading of "Discussion," the article states: "Coupled with the estimates we develop in this paper for the 1982–1983 condor population size (21–24 and 19–22 birds), these comparisons suggest that Koford greatly underestimated the size of the condor population, possibly by as much as a factor of two or three. Furthermore, since the estimate of 40 birds by Miller et al. (1965) was based primarily on comparisons of flock sizes seen by Koford, a similar error factor should probably be applied to the data of these authors as well."

As one of "these authors," and as a long-time acquaintance and co-worker of both Koford and Miller, I find it surprising that the pages of *The Condor* would be used to project so questionable a proposition as is set forth in the above statements. Neither Koford or Miller is here to question this pointed reflection on their work. That fact alone would seem to call for special editorial evaluation. Also reason for critical appraisal would be the long-prevailing, ongoing, and well-known controversy over two different ways to save the California Condor—either as a wild, free-living constituent of its native ecosystem, or as a creature of artificial propagation and domestication.

In that controversy, the rationale for artificial propagation has been based primarily on the claim that the decline of the California Condor population is proof that its natural survival is impossible and, hence, that artificial propagation is the only alternative. Obviously, the more dramatic the decline can be made to appear, the more effective is this rationale.

On the side of natural survival, I have been an active participant in the controversy and have studied various, previous forms of the opposing rationale. None, in my findings, has had sound scientific basis. Because I am well acquainted with the methods and data of Koford's and Miller et al.'s reports, I would be highly skeptical of any suggestion that would change Koford's 1953 estimate of 60 birds by more than five birds, and I would say the same for the 40 birds estimated by Miller et al. in 1965. The proposition that the California Condor population could have numbered 180 birds at the time of Koford's 1953 report and 120 birds at the time of Miller et al.'s 1965 report is, in my view, simply preposterous, and doubly so as it appears in *The Condor* as a featured point of a scientific article.

Except as it includes this incredible proposition, I find nothing questionable about the article. Notably, the proposition appears out of context and quite irrelevant to the rest of the report, which tends more to corroborate and confirm than to discredit the Koford and Miller studies. Why, then, was the odd proposition included?

For members of the Cooper Ornithological Society who may wish to appraise all sides of this question, I submit this commentary for publication. As other related reading, I would like to recommend an article that I prepared several years ago: "Botching the condor program" (1981, pp. 209–216. *In The condor question, captive or forever free.* Friends of the Earth, San Francisco).

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*The Condor* 87:446–447


In the above commentary on our condor censusing article, McMillan has expressed dismay with our discussion of two earlier population estimates—the estimate of 40 birds developed in the study in which he participated during the early 1960s (Miller et al. 1965) and the estimate of 60 birds developed by Koford (1953) for the 1940s. However, he does not provide specific details as to why our discussion might be deficient.

Since our paper presented not only a new photographic method for censusing California Condors, but also the results of the first photographic censuses, we felt it both appropriate and instructive to compare our estimates with those of previous researchers in order to assess the magnitude of the decline in recent decades. Our evaluation of the estimates of Koford and Miller et al. was based on the same kind of extrapolation that Miller et al. used in comparing their figures to those of Koford, but with one significant difference: we were extrapolating from an estimate that can be objectively tied to actual numbers of birds in the wild, while Miller et al. were extrapolating from an estimate (Koford's) that had never been adequately explained. Miller et al. noted an apparent decline of about 30% in high counts of condors from the time of Koford's study to the time of their study, and mainly on this basis concluded that there were about one third fewer birds left in the wild than the 60 birds Koford believed to exist. By comparison, we have been documenting, in the most intensively studied and important condor use areas, high flock counts that are only about one-tenth to one-fifth as large as those documented by Koford in the same regions. As we can be certain of the existence of an average minimum of 20 birds in 1982 and 1983, we might have expected high counts about one-third as large as those Koford obtained if his population estimate had been accurate. It follows that Koford may have underestimated the size of the wild population by a factor of about two or three. Similar arguments apply to the estimate of Miller et al. Because underestimating the rate of decline from the past to the present and on into the future (i.e., underestimating the size of the condor population in the past) would be a non-conservative mistake to make in present efforts to maintain the species, we would have been negligent if we had ignored these matters.

Although McMillan has indicated that he would have difficulty accepting any revision of Koford's estimate by more than five birds, it appears that Koford himself did not regard the figure of 60 birds as beyond significant revision. Koford did not provide any discussion of the accuracy of his population estimate in his 1953 monograph, but in his doctoral thesis of 1951 he remarked (p. 405): "If my estimate of 60 condors is in error, it is too conservative. If there are 100 condors, so much the better for the survival of the species." Since Koford did not consider a population of 100 birds beyond possibility, it is conceivable that he might have supported an even higher estimate if our more recent data had been available to him.

Even with an upward adjustment of the estimates of

Koford and Miller et al., recent events have indicated a much more rapid decline than might have been anticipated. Since publication of the article on the 1982–1983 census, we have completed a 1984 photographic census and have been keeping close photographic track of the remnant population in 1985. The results have not been encouraging. The late summer minimum for 1984 was only 15 birds in the wild, down from 19 in 1983 and 21 in 1982. Since fall 1984, the situation has worsened considerably, as the number of wild condors being documented currently (June, 1985) is only nine, and four of the five breeding pairs known in 1984 are no longer in existence. The intensity of efforts to photograph condors in 1985 has been greatly increased (about doubled) over efforts of previous years, and hopes of finding any of the birds that have recently disappeared are declining rapidly. At the rate of disappearance of the wild population seen in the past three years, and especially in the last year, the population may be gone within another two years.

While part of the recent decline can be attributed to efforts to establish a viable captive population, the major factor has been continuing high mortality owing to largely unknown causes. Almost all captives have been taken as eggs or nestlings, and it is a straightforward process to calculate that the present wild population would include only about four additional birds if the captives had not been taken. The wild population had been averaging only two fledglings per year in the years just before the start of egg removals in 1983. Thus, we might have expected four young to have fledged in 1983 and 1984 in the absence of removals. In addition, two other birds were taken captive in 1982. Applying a recent mortality rate of about 20% per year in the wild to the six birds that have been effectively removed from the wild population would yield an expected four of these birds still alive if they had been left in the wild. As about a dozen birds have disappeared from the wild population during this period, the taking of captives accounts for only about one-third of the decline.

The present status of the California Condor can only be described as one of critical endangerment. The wild population is clearly inviable with respect to its tiny size, excessive mortality rate, and loss of breeding pairs, and there does not appear to be any practical way to make it viable in the short term. Hopes for survival of the species in the years just ahead and for ultimate reestablishment of a viable wild population must center in continuing efforts to achieve a self-sustaining, genetically-adequate captive population, followed by releases of captive progeny to the wild. At present, a captive population consisting of 18 birds exists at the San Diego and Los Angeles zoos. Unfortunately, relatively few family lines are represented in the flock and only a few additional family lines still exist in the wild. Furthermore, the remaining family lines in captivity and in the wild may all be quite closely related to each other. Signs of possible genetic deterioration of the wild population are already evident in the very low levels of heterozygosity that is being found in blood enzyme studies currently underway (K. Corbin, pers. comm.) and in the deficiencies seen in behavior and reproductive performance of most recent pairs. One pair has consistently produced chicks with physical abnormalities, another has exhibited poor hatchability of eggs and viability of chicks, a third has been laying exceedingly tiny eggs (considerably smaller than the smallest eggs recorded previously for the species), and a fourth has had chronic troubles with excessive aggressiveness of the male adult toward his mate, leading to recurrent nesting failure. Another potential sign of recent genetic difficulties has been a biased sex ratio. Since 1982, 14 of 18 condors hatched and sexed have been females, a ratio differing significantly from 50/50. If these problems are indeed indicative of inbreeding depression in the species, true genetic adequacy of the captive population may not be achievable even if all the remaining wild birds are brought into captivity.

With the rapid decline of the wild population that has been seen in the past few years, options for conservation of the condor have become greatly restricted. It is doubtful that the wild population can last much longer no matter what is done on its behalf, and the very effort to sustain the wild population now conflicts with the establishment of a viable captive population. Choices must be made in the near future, and the most conservative strategy appears to be one of not short-changing the captive-breeding approach. Surrogate studies with Andean Condors (*Vultur gryphus*) have demonstrated the feasibility of breeding large cathartids in captivity and releasing them to the wild successfully. There is every reason to believe this approach can work as well with the California Condor. Releases of California Condors to the wild, however, are not likely to be successful until the causes of the excessive mortality of the species have been identified and countered. Success in achieving a viable wild population cannot come quickly and will demand a long-term commitment of research and conservation resources.

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