Quantity versus Quality in California Condor Reintroduction: Reply to Beres and Starfield

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The long-term goal of any species reestablishment program should be the creation of self-sustaining wild populations that replicate as closely as possible the behavior and ecology of original wild populations. We analyzed current reestablishment efforts for the California Condor (*Gymnogyps californianus*) by identifying and discussing both demographic and behavioral problems inhibiting success (Meretsky et al. 2000). We then suggested solutions to these problems. Our demographic model was developed with the clear objective of evaluating the demography of the historic condor population and developing benchmarks for evaluating long-term survival rates of reintroduced birds. Our goal was not, as contended by Beres and Starfield (2001; this issue), a campaign against double clutching of captive pairs; contrary to their allegations, our comments on the implications of multiple clutching for production of parent-reared birds did not stem from results of our demographic modeling. We did not present any chain of arguments resembling that attributed to us in their third paragraph, and our approach does promote practical management decisions leading to self-sustaining and properly behaving populations.

Unfortunately, the alternative condor model offered by Beres and Starfield (which is too incompletely presented to allow an examination of its internal consistency or to evaluate its behavior under alternative assumptions) addresses neither of the primary release problems identified in our paper—demographic unsustainability and failure to achieve appropriate species-typical behavior. Instead, their modeling exercise is evidently driven entirely by an assumption that whatever strategy produces the most condors for release in a given period of time should be used in reestablishment efforts. Such an approach ignores the fact that different rearing procedures can have profound effects on the quality of behavior exhibited by released populations and the fact that rate of production of releasable condors is no guide to the sustainability of released populations. Their analysis is entirely focused on a single process—double clutching—and thus cannot provide management suggestions for countering the major problems that have arisen in the program: unsustainability of the wild population and behavioral problems of released birds.

Problems with Sustainability

The primary problem we discussed in our paper was the excessive mortality rates seen in most releases, rates that far exceeded the 10% annual rate that we calculated would likely produce sustainable populations under normal reproductive conditions. Beres and Starfield assume that mortality rates in future generations of released birds will be lower than those currently observed in the released populations. In contrast, we predicted that mortality rates were likely to increase to approach the disastrous mortality rate of the historical wild population (26.6% annually) because of increasing vulnerability of the released birds to lead poisoning, the primary known stress factor for the historical population.

Our prediction has been quickly confirmed in the southern California and Arizona release programs. From July 1999 through September 2000 (the period since we concluded analyses for our paper), an additional 20 released

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condors died, increasing the overall annual mortality rate in southern California to 23.1% (calculated from December 1993 when releases with aversive conditioning began) and in Arizona to 25.4% (including all releases, beginning in December 1996). Only the central California release program continues without mortalities, likely because of the greater continuing dependency of these birds on clean food subsidies provided for them.

Our prediction of increasing mortality was not based on arguments relating to the practices of puppet rearing or double clutching. Rather, we pointed out that this outcome was likely because the policies favoring termination of the clean-food subsidy for released birds could be expected to result in the same mortality rates that characterized the nonviable historic wild population, because little had been done to eliminate lead ammunition from the release environments or to significantly reduce any other historic mortality factors during the intervening 15 years. Of the recent deaths, 5 have been attributed to lead poisoning, and 10 emergency chelations have resuscitated acutely poisoned birds. Causes of more than 10 of the 20 recent deaths are unknown, but could include additional cases of lead poisoning.

Unlike Beres and Starfield, we harbor no hopes that these mortality rates will decline spontaneously to sustainable levels once a second generation of condors is produced in the wild. All condors have to eat, and this is what makes them vulnerable to lead poisoning, whether they be puppet-reared or parent-reared, experienced or inexperienced. The experienced historic wild population crashed because it had no defenses against such mortality threats. Released populations feeding on lead-contaminated carcasses will meet a similar fate.

The mortality rates currently associated with condor releases in Arizona and southern California are unsustainable and have been progressively worsening, not improving. Management activities and policies to monitor and protect condors clearly must be changed fundamentally to correct this situation, as we discussed in some detail in our paper. Nothing in the approach of Beres and Starfield can be expected to identify problems with survival or to increase the chances for survival of released birds. Under present or unchanged future conditions, it will make no difference how many birds are released to the wild. The populations will remain unsustainable and will ultimately crash to extinction if releases are terminated. Many condors will die needlessly in the process.

The value of a model to predict the future success of a reintroduction program depends on the validity of its assumptions and the accuracy of the demographic rates assigned to released birds. The Beres and Starfield model predicted the size of the wild condor population decades into the future under the assumption that mortality rates would achieve sustainable levels that wild condors likely have not experienced in decades or perhaps centuries (Snyder & Snyder 2000). The losses in the past year (20 birds) have nearly matched the entire production of condors by the captive breeding program during the same period (22 birds). Should such losses continue, the whole release enterprise could collapse. The graphs the authors present, purporting to show relatively stable populations well into the present century, assume that the theoretical mortality rates we calculated to be necessary for population stability will occur spontaneously in the future. There is presently no justification for such an assumption.

Behavioral Problems

We also presented considerable information on the behavioral problems encountered to date with puppet-reared condors. In repeated releases over the past 12 years, such condors, even with aversive conditioning, have consistently shown excessive tendencies to approach humans and human structures, have sometimes received food handouts from bystanders, and have repeatedly vandalized human property ranging from tents and sleeping bags to screen doors, vehicle windshield wipers, mattresses, and roof shingles. Such behavior was not known in the historic wild population; at most, fledglings were relatively approachable around their nests. Moreover, such behavior has shown no clear tendency to disappear in released populations and may represent a permanent characteristic of the populations involved.

Because of the strong social tendencies of the California Condor, new birds released into such populations are likely to learn the misbehaviors characteristic of their comrades, regardless of what prerelease training they may have received. The transmissibility of bad behaviors lies at the heart of our concern about prolonging release efforts that are resulting in chronic misbehaviors. If, as we believe probable, misbehaviors do not spontaneously disappear from afflicted released populations, it will make no difference how many birds are released into such populations. The program will not achieve the goal of producing properly behaving wild populations. The analysis of Beres and Starfield does not address this problem.

Only the parent-reared birds released in isolation in the Ventana Wilderness Area have shown clear signs of better behavior, and this suggests that excessive human-oriented behavior is not inevitable in releases. Because of these results and the superior results achieved with parent- and wild-reared stocks in other release programs (e.g., Griffith et al. 1989; Snyder et al. 1994), we believe that the most risk-averse approach would be to remove misbehaving populations from the wild and to restart releases with birds that have been parent-reared under the most natural conditions possible and that are kept fully

isolated after release from birds afflicted with human-oriented behavior. These birds should be raised by their parents in naturalistic field enclosures, minimizing as much as possible their familiarity with humans and human structures prior to release. Further, sources of unsustainable mortality should be controlled at the earliest opportunities, and management practices available to reduce their impact should be firmly in place until these factors can be eliminated.

We also recommended maximizing production of parent-reared birds for release, even though such a goal involves penalties in numbers of birds that can be released relative to the numbers that can be released via puppet rearing; one cannot maximize parent rearing and multiple clutching simultaneously under present procedures. This is not to argue that there is no role for multiple clutching in the captive breeding program, and we have not called for total abandonment of the technique.

Data from the captive flock indicate that approximately one-third of the pairs from which first eggs have been removed have not produced second eggs. Thus, maximal double clutching of the captive flock cannot be expected to double captive production as assumed by Beres and Starfield. Furthermore, assuming that all second eggs might be parent reared, whereas all first eggs taken from pairs would have to be puppet reared, the output of parent-reared birds under double clutching would be substantially less than what might be obtained by parent rearing of all first eggs. Thus, under current procedures, maximal double clutching significantly reduces potential numbers of parent-reared offspring.

Near-maximal multiple clutching was a crucial strategy in the formation of a captive flock in the 1980s (Snyder & Hamber 1985). It continued to be crucial in the early stages of captive breeding, when it was important to increase the size of the captive population rapidly and to ensure that adequate genetic representation of family lines was achieved before some of these lines might be lost. The latter goals, however, were short-term goals that have been largely achieved, and now we suggest that approaching maximum numbers of parent-reared birds for release, by whatever means possible, should take priority, and that releases of puppet-reared birds to the wild should be terminated. Although some double clutching may continue to be useful in the captive flock for a variety of reasons not directly related to release, maximal multiple clutching no longer serves the overall goals of the program.

Conclusions

Beres and Starfield have introduced an incompletely elaborated model to counter arguments we did not make and have failed to address the major difficulties of the release program that form the core of our paper. Furthermore, they have reduced the complexities of the condor program to a game that they apparently assume is won by maximizing productivity in captivity. We cannot agree with their characterization of our paper and the model we presented, and we see no value in basing overall decisions in the condor release program on their demographic strategy, which favors quantity and ignores quality and survival of released birds. The goal of the release program must remain that of self-sustaining and properly behaving wild populations. Blind pursuit of maximum captive propagation undermines progress toward creating self-sustaining and properly behaving populations, as discussed at length here and in our paper.

Literature Cited

Beres, D., and A. M. Starfield. 2001. "Demography of the California Condor" and management decisions based on modeling: a reply to Meretsky et al. Conservation Biology **15:**1445–1448.

Griffith, B., J. M. Scott, J. W. Carpenter, and C. Reed. 1989. Translocation as a species conservation tool: status and strategy. Science **245:**477–480.

Meretsky, V. J., N. F. R. Snyder, S. R. Beissinger, D. A. Clendenen, and J. W. Wiley. 2000. Demography of the California Condor: implications for reestablishment. Conservation Biology **14:**957–967.

Snyder, N. F. R., and J. A. Hamber. 1985. Replacement-clutching and annual nesting of California condors. Condor **87:**374–378.

Snyder, N., and H. Snyder. 2000. The California Condor: a saga of natural history and conservation. Academic Press, San Diego.

Snyder, N. F. R., S. E. Koenig, J. Koschmann, H. A. Snyder, and T. B. Johnson. 1994. Thick-billed Parrot releases in Arizona. Condor **96:** 845–862.