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EVALUATING NORTHERN GOSHAWK (*ACCIPITER GENTILIS ATRICAPILLUS*) POPULATION STATUS: A REPLY TO SMALLWOOD AND CROCKER-BEDFORD

PATRICIA L. KENNEDY
Department of Fishery and Wildlife Biology and Graduate Degree Program in Ecology, Colorado State University, Ft. Collins, CO 80523 U.S.A.

Shawn Smallwood and Cole Crocker-Bedford present thought-provoking reviews of my recent paper on Northern Goshawk (*Accipiter gentilis atricapillus*) population trends (Kennedy 1997). In addition, Crocker-Bedford provides a detailed review of his controversial 1990 paper on forest management and its impact on goshawk reproduction (Crocker-Bedford 1990). Finally, both authors present their ideas on alternative approaches that might be used to evaluate the status of the goshawk. Here is my reply to their comments.

OBJECTIVE OF KENNEDY (1997)

Smallwood and Crocker-Bedford find fault with my paper because I did not include habitat analyses. They rightly claim that evaluating habitat loss is a key listing criterion of the Endangered Species Act (ESA). I do not disagree with them and think a thorough analysis of goshawk habitat data is an important component of a status review. But the aim of my paper was not to conduct a status evaluation for the listing proposal, which was clearly misunderstood by the two authors. A status review is the purview of the U.S. Fish & Wildlife Service (USFWS) and they just finished such an evaluation (Clark 1998). I merely evaluated the petitioners' claim "that goshawk populations have suffered significant declines." I wanted to see if the statements presented by the petitioners as fact indeed had empirical basis. I treated their statement as an hypothesis, proceeded to test this hypothesis, and found no support for their statements.

The goal of my paper was to conduct the first step in a status assessment and determine, in a scientifically thorough manner, if there is evidence of a population decline. I did not continue to the next step, that of determining reasons for a decline, because, as I stated in my paper, "Diagnosing a cause of decline is irrelevant if there is no evidence that a decline has occurred." Once some evidence of a decline has been documented then the cause(s) of the decline can be determined and appropriate conservation plans developed and implemented (Caughley and Gunn 1995). If there is no evidence of a demographic decline, how can we justify spending taxpayer dollars to develop and implement expensive recovery programs? Without demographic data, how does the recovery team establish achievable, quantifiable recovery goals as delisting criteria (see Pagel et al. 1996, Cade et al. 1997, and Pagel and Bell 1997 on the debate about recovery goals for American Peregrine Falcons [*Falco peregrinus anatum*])? The USFWS used a similar approach in their recent status evaluation where they examined evidence that goshawk populations were declining and then proceeded to evaluate the potential loss of goshawk habitat. They concluded that listing the goshawk as Endangered or Threatened west of the 100th meridian is not warranted (Clark 1998).

WHAT RESPONSE VARIABLES ARE APPROPRIATE TO EVALUATE GOSHAWK POPULATION TRENDS?

Evaluating Goshawk Trends Using Demographic Variables. There are two general approaches that can be used to monitor population trends: the survey method and the demographic method (Taylor and Gerrodette 1993). Using a survey method would entail attempting to estimate population size (or some index of population size) directly over several years and determine whether or not the estimates indicate a decline over time. Because it is not feasible to census the entire population of most bird species (including raptors), population monitoring is almost always based upon surveys of a sample of the population. The demographic method involves monitoring trends in vital rates (survival, fecundity, immigration, and emigration) and then using these data to calculate finite population growth rate (λ). λ can be calculated by following reproduction and survival of individual cohorts (age classes), or it can be estimated through simulation based on annual variation in cohort survivorship and reproduction (Gotelli 1998).

In my paper, I examined available data that could be used to monitor goshawk population trends, using either the survey or demographic approach. Surprisingly, neither author thought any of the demographic response variables I chose to evaluate trends was useful for determining goshawk population status! In his conclusions, Smallwood states: "Population density, fecundity, survival and rate of population change all lack scientifically defensible relationships with range-wide abundance. . . ." I disagree and still adhere to the basic tenets of population biology that I describe here. Attaining accurate measurements of these parameters that are appropriate for the scale of inference, however, is problematic but not impossible.

Population Abundance. Abundance refers to the number of individuals within a population (or population size) (Krebs 1994). If the population is so large that a study cannot encompass the whole of it (e.g., range-wide goshawk population), then abundance must be presented in terms of densities rather than absolute numbers. Thus samples are taken and abundance is expressed as number of animals per unit area (Begon and Mortimer 1986). Density is thus the spatial expression of abundance (Krebs 1994). Temporal trends of density would reflect temporal trends in abundance. As indicated by Smallwood, simple tallies of nests to estimate breeding density in a study area is fraught with problems and produces biased estimates of population size (Gould and Fuller 1995). Rather than rejecting density as an appropriate response variable, this problem could be solved by estimating population size using Jolly-Seber models. The Jolly-Seber model is a capture-recapture model allowing for an open population in which additions and/or deletions occur. The model produces population density estimates for each sampling period (e.g., year). This method has been described extensively in the literature and the application of this approach to raptors is described in an excellent paper by Gould and Fuller (1995).

Another potential approach for monitoring abundance of goshawks is the area-occupied technique (Iverson and Fuller 1991). This approach employs repeatedly broadcasting calls from the same locations, and using the pattern of responses to estimate the probability of detecting an animal given that one is present. Probability of detection—area occupied techniques have been used successfully on another woodland raptor, the Red-shouldered Hawk (*Buteo lineatus*, McLeod and Andersen in press), and are particularly promising for monitoring species in landscapes where proportion of area occupied is high, and birds have a high probability of responding to a call. To date, little work with this technique has been conducted with goshawks. However, goshawks respond to call broadcasts (Kennedy and Stahlecker 1993); thus, this approach may be useful in monitoring their populations.

Before this technique could be applied widely, it would need to be validated in areas where goshawk density has been estimated independently. Currently, the relationship between estimates of area occupied and breeding density have not been clearly established; so, before this technique could be used to monitor breeding density, such a relationship would have to be evaluated. Bart and Robson (1995) describe a double-sampling procedure that could be used to calibrate this technique. Density could be estimated on quadrats using foot (Rosenfield et al. 1998) or aerial surveys for occupied nests (aerial surveys could only be used before leaf-out in deciduous habitat [see Cook and Anderson 1990 for an example]). These estimates would be compared to the estimates obtained from the area-occupied technique and the area-occupied estimates would be adjusted accordingly.

Vital Rates. The population attributes (or vital rates) influencing changes in abundance are immigration and birth, which increase abundance, and emigration and death which reduce it (Begon and Mortimer 1986, Krebs 1994). The combined effect of these four processes provides an accurate indication of how abundance changes. λ potentially can be estimated with a high degree of precision and accuracy. Both authors criticize the use of these demographic variables because of sampling difficulties. Crocker-Bedford states that ". . . it is usually impossible to calculate a meaningful λ for a sparsely distributed species." This is not true. Meaningful λs have been calculated for several species of management concern including the Northern Spotted Owl (*Strix occidentalis caurina*, Burnham et al. 1996) and Ashy Storm-petrel (*Oceanodroma homochroa*, Sydeman et al. 1998). I agree with Crocker-Bedford that this parameter is difficult to estimate particularly when using capture-recapture data to estimate survival. However, survival rates can be determined using other methods such as radiotelemetry (Iverson et al. 1996, Ward and Kennedy 1996, Ganey et al. 1998). The estimation procedure is less complex than for banding data (see White and Garrott 1990) and I hypothesize that smaller sample sizes would be required than with capture–recapture data, although I have not conducted a power analysis to test that hypothesis.

Contrary to the criticisms of both authors, I still think a metaanalysis would be useful to estimate goshawk fecundity and survival from currently available vital rate data collected at individual study areas. This approach was used successfully to analyze Northern Spotted Owl datasets (Burnham et al. 1996) so there is no reason why this approach could not be used for the goshawk which is a species that is more widely distributed and probably more abundant than the Northern Spotted Owl. This metaanalysis would be an inexpensive next step to determine what types of data are needed and how many study areas would be required to obtain sufficient data. For example, using this approach, the datasets of Reynolds and Joy (1998) referred to by Crocker-Bedford could be pooled with the survival data presented in my paper and in DeStefano et al. (1994). Because sampling protocols were similar in all three study areas, survival estimates could be analyzed for the years in which the studies overlapped (1991–92 for all three studies and 1991–95 for New Mexico and Arizona). This should be done before more resources are committed to collecting vital rate data and the results of the analyses could be used to assist the design of future long-term studies. I agree with Smallwood that a metaanalysis should not be used in lieu of proper sampling. However, it is an underutilized tool that can be used to analyze data from multiple, well-designed, coordinated studies which are unlikely to estimate population trends individually due to the rarity of the species.

Evaluating Goshawk Trends Using Migration Counts. Smallwood suggests that goshawk abundance should be evaluated based on changes in migratory counts. The utility of migration counts for monitoring population trends has been much debated (see Bildstein 1998 for a detailed discussion of the strengths and weaknesses of migration counts as an index to population size). To track population change, a constant proportion of the index (e.g., migration counts of goshawks) to the true population size must be maintained. If this does not occur then the proportion must be estimated. These validation studies have not been conducted on the goshawk for a local area or range wide, so the trends in the current migration count data are difficult to interpret.

Also, trends in migration counts could reflect distributional changes or changes in residency patterns rather than changes in population size. For example, recent analyses of Christmas Bird Count data suggest that Sharp-shinned Hawks (*A. striatus*) are increasing. Several authors have suggested that more Sharp-shinned Hawks are overwintering in northern North America because of warmer winter climates and/or the abundance of bird feeders which provide a stable overwinter food source (see review in Bildstein 1998). This could be the reason that counts of Sharp-shinned Hawks at northern migration stations have been lower in recent years. Since goshawk migrations are characterized by irruptive invasions, migration counts of this species are more likely to reflect residency patterns than changes in abundance (Bednarz et al. 1990, Titus and Fuller 1990). So, in response to Smallwood, to replace demographic variables that are known to represent abundance or influence abundance with an uncalibrated index is inappropriate. However, migration counts could be continued and used as an addendum to demographic studies to determine if the counts reflect demographic changes in goshawk populations.

Evaluating Goshawk Status by Monitoring Habitat Variables. What is the role of monitoring habitat variables in determining the status of goshawks? I agree with Smallwood, Crocker-Bedford, and DeStefano that habitat variables should be included in a goshawk-monitoring program. However, as noted by Crocker-Bedford and DeStefano, habitat monitoring should augment demographic studies, not replace them. Evaluating goshawk status purely from migratory counts and information on habitat availability and contiguity as suggested by Smallwood assumes that goshawk habitat can be defined and that the relationship between these variables and goshawk abundance is well-documented. Currently, these relationships are not well-defined.

In the recent status evaluation the USFWS concluded, "The information presented in the petition relies largely on the contention that the Northern Goshawk is dependent on large, unbroken tracts of 'old-growth' and mature forest. However, the Service has found no evidence to support this claim. The Service found that while the goshawk typically does use mature forest or larger trees for nesting habitat, it appears to be a forest generalist in terms of the types and ages of forests it will use to meet its life history requirement. Goshawks can use small patches of mature habitat to meet their nesting requirements within a mosaic of habitats of different age classes . . ." (Clark 1998). I concur with their findings and suggest that more habitat studies are needed that are designed to determine the range of habitats used by the goshawk. I agree with Smallwood, Crocker-Bedford, and DeStefano that these studies need to be conducted at multiple spatial scales to be meaningful. I would add that habitat studies should be conducted year-round and not just focused on nesting habitat. Our knowledge of goshawk winter ecology is appallingly scant (Squires and Reynolds 1997). Finally, I concur with DeStefano that trends in forest habitat availability should also be documented to determine trends in availability of goshawk habitat.

Once goshawk habitat is well-defined and demographic data are available from several study areas for an analysis of population trends (see DeStefano for further discussion of the value of long-term studies at multiple study areas), I'd recommend we begin development of a model (or models) that predicts the relationships between suitable nesting and winter habitat and population trends and/or performance. This predictive model will need to be refined and tested to examine relationships between habitat data and population size or other relevant demographic parameter. If a habitat model can predict goshawk population performance, then monitoring programs can switch emphasis from population-based monitoring to habitat-based monitoring. If habitat models do not adequately predict population performance, population-based monitoring will need to be continued and the habitat relationship information will need to be reevaluated.

This approach is based on ideas presented by recent monitoring plans for the Marbled Murrelet (*Brachyramphus marmoratus*, Madsen et al. in press) and Northern Spotted Owl (Lint et al., in press) in the Pacific Northwest, and monitoring plans for the goshawk in the western Great Lakes region (Kennedy and Anderson unpubl. data). The emphasis is to use the demographic and habitat data collected in the initial phases (Phase I) of a monitoring program and to develop habitat-based models that use habitat features to predict goshawk occurrence and demographic performance in the latter phases (Phase II) of a monitoring program. If reliable habitat models can be developed to predict population status and trend at a landscape scale, monitoring can switch from intensive and costly population-based monitoring to a less expensive habitat-based monitoring approach. The habitat-based monitoring would emphasize monitoring the habitat features that predict goshawk performance and/or status, with less emphasis on monitoring population parameters. However, presence/absence of breeding goshawks in suitable habitat (as identified by the habitat models) would need to continue in Phase II to ensure that this habitat remains occupied. I emphasize that the switch from Phase I to Phase II can only occur if the habitat models are demonstrated to reliably predict goshawk population performance. Models that are not validated are essentially equivalent to untested hypotheses, so population-based monitoring would have to continue until validated models are developed.

In addition to the model development, I strongly support DeStefano's suggestion that on-site experiments designed to measure goshawk responses to silvicultural treatments be initiated. These quasi-experiments are being implemented continuously in the form of timber harvest near goshawk nests; most sale areas are identified years before the sale allowing for the collection of adequate pretreatment data. Monitoring pre- and posttreatment movements of even a few pairs of birds would provide us with fascinating qualitative insights into goshawk responses to harvest and could be the basis for designing additional experiments.

Crocker-Bedford does not think field experiments like this are possible and states, "Scientists should explicitly recognize that goshawk field studies are correlative. . . ." I disagree with this statement because these types of landscape-level, quasi-experiments have been conducted on passerine communities (Bierregaard and Lovejoy 1989, Schmiegelow et al. 1997) and goshawks have been successfully used as experimental units in field experiments (Kenward et al. 1993, Ward and Kennedy 1996, Dewey 1998). Thus, we are not restricted to correlative studies. Although correlative studies are valuable in identifying patterns, they do not imply cause and effect (Romesburg 1981, 1989, Krebs 1994). For example, trends in population or habitat availability do not imply causes of population change; experimental data are needed for such an evaluation. Raptor biology can move beyond its dependence on the correlative approach and toward more field experimentation with creative thinking about how to test hypotheses and a willingness to try new approaches. Romesburg (1981) claimed nearly two decades ago that much wildlife science was compromised with respect to providing the reliable knowledge required to make management decisions. He argued that management should be based on "good science," which is the scientific evidence best able to provide reliable knowledge. Reliable knowledge is based on the hypothetico-deductive (H-D) method. The H-D method employs three steps. observation/induction, hypothesis formation and experimentation (Romesburg 1981, 1989). Crocker-Bedford is arguing that we approach goshawk management by only completing the first two steps. What typically happens when this is done in management is hypotheses advanced to account for observations gradually evolve into explanations for them through a process Romesburg (1981) called retroduction. The petitioners' statements about goshawk declines are examples of retroduction.

COMMENTS ON CROCKER-BEDFORD (1990)

In addition to providing a thoughtful critique of my paper, Crocker-Bedford dedicates a considerable segment of his rebuttal detailing methodologies and strengths and weaknesses of his controversial 1990 paper. He is providing these details to rebut recent scientific evaluations of his 1990 paper (Kennedy 1997, White and Kiff 1998). Crocker-Bedford's identification of the strengths and weaknesses of his 1990 paper adds a valuable component to this scientific debate and an appropriate addendum to his 1990 paper. However, I disagree with several points he makes.

As I mentioned in my paper (Kennedy 1997), one of the major strengths of Crocker-Bedford's 1990 paper was that it was the first published paper to suggest that goshawk populations were declining due to overharvest of their forested nesting habitat. This idea was important and it fueled this stimulating debate on goshawks and forest management. However, his paper has some serious flaws. Crocker-Bedford implies that his study was criticized because he had conclusions that were politically sensitive. It is likely that some of the unpublished criticisms he received over the years were politically motivated, yet the aforementioned published critiques were based on scientific merit.

Crocker-Bedford claims that one of the strengths of his paper is that he ". . . demonstrated long-term nest tree fidelity in the absence of habitat degradation." Whether or not he demonstrated this depended on his methods for estimating locale reoccupancy, which have still not been adequately explained. Comparisons of occupancy rates need to be done cautiously because occupancy rate is a subjective parameter that is probably correlated with the amount of effort expended to determine territory status (White et al. 1995, Kennedy 1997). We still do not know if Crocker-Bedford (1990) used standard search effort techniques for treatment and control locales. He states that his study was not biased by an inappropriate nest search effort and justifies this based on his large sample size. However, sample size is not the major factor influencing estimation of occupancy rates, it is search effort. He states "... the vicinity was extensively searched for alternate nests." Was each nest site searched with equal effort and was an equal-sized area searched prior to determining a site was unoccupied? This is important because there is a high probability of missing alternative nests in goshawk territories due to large inter-alternative distances. In California, mean distance between alternative nests was 273 m and the range was 30–2066 m (Woodbridge and Detrich 1994). In Arizona, mean inter-alternative nest distance was 489 m and the range was 21–3410 m. Approximately 89% of alternative nests in Arizona were within 900 m and 95% were within 1400 m of one another (Reynolds and Joy 1998). Clearly, the potential for misclassifying an occupied territory as unoccupied is great if nest site searches are restricted to the immediate vicinity (50–100 m) of the most recently used nest. So small search areas, even if they are consistently applied to treatment and control locales, might result in more false negatives in treatment locales because harvest might influence inter-alternative distances rather than occupancy rates.

The most controversial statement in Crocker-Bedford (1990) was his claim in his summary that the goshawk population on the North Kaibab Ranger District declined, "... from an estimated 260 nesting pairs in 1972 to approximately 60 pairs by 1988." He claims that his breeding population projections are one of the strengths of his paper. I strongly disagree because I think this statement is an example of inappropriate inference given his dataset. He did not provide an analysis of the limitations of his calculations nor did he provide alternative explanations for his results. He based his estimation of rate of population change solely on published breeding density estimates of the areas harvested in the 1950s and 1960s (Crocker-Bedford and Chaney 1988) and his estimates of reoccupancy rates (Crocker-Bedford 1990). He cites unpublished data in this rebuttal that were apparently used in these calculations. However, the methods he used for estimating these densities are unknown and should have been presented in the 1990 paper. In addition, he did not estimate a variance of any of his density estimates, which influences one's interpretation, as I will demonstrate below.

Crocker-Bedford argues that his breeding population projections are corroborated by recent population size estimates of the same area by Reynolds and Joy (1998). In contrast, I suggest that the Reynolds and Joy (1998) results provide an excellent example of why his projections were an example of inappropriate inference. Reynolds and Joy (1998) estimate that approximately 100 territories currently remain on the District (they have located 95 occupied territories in surveys of 95% of the District). Crocker-Bedford estimated the population size in 1988 to be 60 pairs. If we take a conservative approach and assume the population size has not increased between 1988–96, this suggests that Crocker-Bedford's estimate of 60 pairs could vary by 66% (20–100 pairs). If we extend this simple estimate of variance to his historical estimates they could have varied from 86–432. We cannot compare these ranges statistically because we do not know his estimate of variance, but these calculations suggest that one plausible breeding projection would be that the number of pairs varied between 86–100 between 1972–88, respectively. This is equally as plausible an interpretation as the one provided by Crocker-Bedford (1990).

Crocker-Bedford (1990) used two estimates of density that may or may not be comparable, depending on the estimation procedures, did not provide an estimate of the precision and bias of his estimator, drew a line through these two points, made a single interpretation of the trends and ignored any plausible alternative explanations. This is considered inappropriate inference within the scientific community. Crocker-Bedford (1990) should have concluded that it was not possible to determine if the North Kaibab goshawk population was increasing, decreasing, or stable because of wide variation in demographic estimates. Maguire and Call (1992) reached similar conclusions in their population viability analysis (PVA) for the same goshawk population. They found, "... the range of variability in parameter estimates, particularly for mortality rates, was so great that our simulation results produced populations that ranged from rapidly increasing to rapidly declining. We are unable to conclude from these results whether the North Kaibab Ranger District is stable, increasing or decreasing." Smallwood incorrectly interpreted their study (cited as Maguire 1993) by focusing on the potential for population declines as a result of habitat loss. However, this was not the major conclusion of the Maguire and Call PVA.

CONCLUSIONS

Although neither Crocker-Bedford nor Smallwood can provide empirical results to refute my conclusions or the conclusions of the USFWS status review, their papers provide thoughtful and insightful comments that have stimulated an interesting discussion about approaches for evaluating population trends in goshawks. The disagreement and controversy described by Smallwood and Crocker-Bedford and expanded by DeStefano are characteristics of intellectual ferment driven by the best creative effort of ecologists and are among the reasons why conservation biology and wildlife management are such exciting fields. I hope these discussions continue and they result in improved approaches to evaluating population trends of rare and uncommon species.

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DETERMINING THE STATUS OF NORTHERN GOSHAWKS IN THE WEST: IS OUR CONCEPTUAL MODEL CORRECT?

STEPHEN DESTEFANO
U.S. Geological Survey, Arizona Cooperative Fish and Wildlife Research Unit, 104 Biological Sciences East, University of Arizona, Tucson, AZ 85721 U.S.A.

In federal district court in Tucson, Arizona recently, a case was heard regarding the status of the Endangered Cactus Ferruginous Pygmy-owl (*Glaucidium brasilianum cactorum*) and development in the Tucson basin (Defenders of Wildlife vs. Amphitheater School District). The western population (Arizona) of the Cactus Ferruginous Pygmy-owl had been listed in 1997 under the Endangered Species Act (ESA), and a local school district wanted to build on an area allegedly used by one or more owls. Defenders of Wildlife, as the plaintiff, was suing to stop the development. Owls had been seen just north and south of the boundary of the property in question, and the attorney for the defense built part of her case on the fact that an owl had not actually been seen inside the property boundary. She used this "uncertainty" about the owls' use of the property, as well as other aspects of its little-known ecology in Arizona, to her advantage and stated in court "there comes a point where the