United States Department of the Interior

U. S. GEOLOGICAL SURVEY

Biological Resources Division
Forest and Rangeland Ecosystem Science Center
Department of Fish & Wildlife Resources
University of Idaho
Moscow, ID 83844-1136

November 8, 1997

Dr. Chris Servheen
Grizzly Bear Recovery Coordinator
U.S. Fish & Wildlife Service
University Hall, Room 309
University of Montana
Missoula, MT 59812

Dear Chris,

I have some comments and concerns that I would like to share with you regarding the document entitled "Further information on specific issues relating to the grizzly bear recovery plan." My comments are restricted to the section entitled "Further information on methods to monitor grizzly bear populations," although some comments pertain to related information presented in the 1993 Grizzly Bear Recovery Plan (US Fish & Wildlife Service 1993).

You reach some very important conclusions in your report. For example (my emphasis added in italics):

Page 35 — "The use of these new monitoring methods on the Yellowstone ecosystem population data allow a comparison of the *accuracy* and *effectiveness* of the Recovery Plan monitoring methods to judge the status of grizzly bear populations."

Page 45 — "Further *confirmation* of the estimators is afforded by the similarity in population growth estimated by various estimators and those obtained by a demographic analysis of survival and fecundity."

Page 47 — "The application of newer methods ... *clearly show* that the monitoring parameters and the target values for those monitoring parameters ... *are accurate* in assessing the status of a grizzly bear population."

Page 47 — "The use of new methods *confirms* that the Recovery Plan monitoring methods *are conservative* and provide an *accurate* and *sound* way to monitor the status of a grizzly bear population."

Page 47 — "...these criteria are *adequate* objective and measurable criteria."

These are formidable conclusions for any scientific basis to live up to. Such conclusions also require clearly articulated standards and sufficient analysis related to the issues of effectiveness, risk, accuracy, and adequacy.

My input thus focuses on two facets of your justification for the adopted or proposed methods and metrics to monitor grizzly bear populations: (1) the scientific basis; and (2) the procedure by which you reached conclusions regarding specific applications to management. What follows could be characterized as grave concerns; but they are offered in the spirit of helpful criticism. In keeping with this tone, I conclude with some recommendations for improving the scientific basis and specific applications of monitoring methods.

1. The Scientific Basis

You cite several scientific papers and technical reports to justify your conclusions that (i) the Yellowstone population is, indeed, increasing at a substantial rate and that (ii) unduplicated counts of females with cubs-of-the-year provide a sound basis for monitoring trend (implied in your use of these counts to calculate finite rate of population increase) and provide a sound basis for estimating minimum or total population size as a referent for calculating total allowable mortality.

Specifically, these papers are: Eberhardt et al. (1994), Eberhardt (1995), Boyce (1995), and Boyce et al. (1998). Estimates of population growth rate presented in each of these papers, or presented in your report using data from Boyce et al. (1998), are subject to substantial and largely undisclosed biases. Most importantly, **in all cases these biases result in an over-estimate of population growth rate**. The details of problems with each of these papers are:

Eberhardt et al. (1994). — The authors estimated population growth rate without censoring the denominator of survivorship calculations for times that bears were not radio-marked (i.e., at risk). This error lead to an over-estimate of population growth rate. This error is pointed out by Hovey and McLellan (1996) and acknowledged by Eberhardt (1995). **This estimate is substantially biased and is not a legitimate point of reference for confirmation of any other growth rate.**

Eberhardt (1995). — In this report, the author corrected the mistake of the previous paper. He also selected a subset of the available demographic data to estimate population growth rate. Specifically, he used only data from 1983-1994 that were obtained from "research-trapped" bears. Data for 1976-1982, a period of widely-acknowledged population decline, were excluded, as were data from "management-trapped" bears. Management-trapped bears are roughly equivalent to bears that have lost their fear of humans and have therefore become problems for managers. We know that this segment of the population has a much lower rate of survival compared to wary (≈ research-trapped) bears (e.g., Mattson et al. 1992). Eberhardt (1995) thus selected an inherently optimistic subset of the data to estimate population growth rate.

This selection of a subset of the data is not necessarily problematic if one assumes that the population by which we are going to judge "recovery" will be restricted only to wary (= research-trapped) bears, and if none of the wary bears ever become habituated. In fact, neither of these assumptions is warranted. All bears, wary or not, are used to judge status of the population relative to recovery criteria (US Fish & Wildlife Service 1993); we also know that wary bears are chronically lost to the habituated (= management-trapped) segment of the population. In such a source-sink structure bears are lost from the source population (wary bears) in two ways: (i) by death and (ii) by recruitment to the sink (habituated) population. Eberhardt (1995) only calculated the rate at which bears were lost due to deaths and neglected to calculate the rate at which research-trapped bears were lost to the "management-trapped" segment of the population. Eberhardt (1995) therefore over-estimated population growth rate for the source population. **As with the previous estimate, the estimate presented in this paper is substantially biased and is not a legitimate point of reference for confirmation of any other growth rate. This conclusion holds doubly so because the estimated growth rate does not apply to the "population" that is being drawn upon to judge recovery.**

Boyce (1995). — This author used essentially the same data as did Eberhardt (1995) and also overlooked calculating the rate at which bears were lost from his source population to the unanalyzed sink population. As with the previous analysis, even if the calculated rate were less biased, it would still have limited relevance to judging recovery because the analyzed population (source) does not correspond to the population (both source and sink) used to count towards recovery criteria. Thus, **the estimate of population growth rate presented in this paper is not a legitimate point of reference for confirmation of any other growth rate nor is it a basis for judging the status of the population of concern to judging recovery.**

> As an aside, these last two reports have not been rigorously peer-reviewed. Dr. Craig Pease and myself have submitted a paper to a prestigious professional journal, that has hitherto fared quite well in the review process, detailing all of these problems. Our paper also offers an estimate of population growth rate for the Yellowstone grizzly bear population that accounts for all of the potential problems outlined above and corrects for numerous other biases, not discussed here, that additionally affect Eberhardt (1995) and Boyce (1995). If the review proceeds as I anticipate, our published results should be available within the next year.

Estimates of growth rate based on data reported in Boyce et al. (1998) — At this juncture it is important to note that none of the above-cited estimates of population growth rate, based on analysis of demographic data from radio-marked bears, provides a basis for confirmation. At most, one would expect that any less biased and management-relevant estimate should be substantially lower. Thus, the estimates of population growth rate presented on Page 45, based on unduplicated counts of females with cubs-of-the-year, need to stand on their own.

As a first point, **there is an error in the information presented at the top of Page 45;** these estimates of finite rate of population growth (λ) are for the years 1986-1996, <u>not</u> 1986-1994 (see Boyce et al. 1998). In fact, λ calculated from data in Boyce et al. (1998; maximum likelihood estimator) for several different periods is as follows:

Period	λ
1986-1993	1.01
1986-1994	1.00
1986-1995	1.02
1986-1996	1.07

Thus, the value of λ for the period 1986-1996 appears to be anomalous compared to values of λ calculated for the previous 3 time periods. In this light, **it is potentially misleading to present λ calculated only for 1986-1996**. These figures also point to the potential volatility of calculating λ in this way from these data.

As a second point, **the annual estimates of total population size based on cumulative counts of females with cubs-of-the-year (COY) in Boyce et al. (1998) are not a reliable basis for calculating population growth rate or, relatedly, monitoring trend.** The authors make this point on page 15 of their draft manuscript: "We caution that use of cumulative counts for estimating grizzly bear populations should not be considered sufficient for monitoring trend..."

There are other reasons, not investigated or disclosed by Boyce et al. (1998), why the models that they present are not a reliable basis for monitoring trend or calculating allowable mortality. **A single key assumption underpins use of all the methods presented in this paper: that individual bears are seen at random, both within and among years.** This assumption is recast in various ways; *e.g.*, "...all bears have the same probability of being sighted, given that a sighting occurs"; "...a constant rate of sightings"; etc. Another way of understanding this assumption is that the rate or the conditional probability for detection of females with COY is a function solely of the number of females with COY in the population in a given year. This does not allow for major intervening effects due to differences among years in search effort by humans or sightability of females with COY.

The authors acknowledge that the validity of this assumption is crucial to the validity of the models. They cast this problem in terms of heterogeneity of detectability among females with COY *within years*. They conclude that, for the maximum range of heterogeneity considered (a coefficient of variation [CV] = 0.4), the within-year effects are minor. **There are two major problems with the appraisal of heterogeneity in this paper: (i) the magnitude of variation that was considered was very likely much too small; and (ii) among-years heterogenity, and multi-annual trends in probabilities or rates of sightings due to search effort and sightability, are of much greater consequence than is within-year heterogeneity.**

Boyce et al. (1998) assert that a 9× difference in detectability among females with COY associated with CV=0.4 "...represents a high level of heterogeneity." It strikes me that for such an important issue it is not sufficient to merely assert some relevant level of heterogeneity. It is probably critical to estimate it. The model developed by Dr. Craig Pease and myself could be used for such as estimation. It is also enlightening to look at the probability that a radio-marked female with COY would be seen during an aerial overflight as a function of the feeding activity that was occurring. Preliminary estimates of such probabilities for Yellowstone grizzly bears suggest that this value ranges from near 0 to near 1 depending on the activity — a range approaching ∞×. Activities such as foraging on mushrooms or fishing for spawning trout have an attendant sighting probability approaching 0, while foraging on army cutworm moths in alpine cirques stands out from all other activities by having a sighting probability approaching 1 (i.e., the bear is almost certain to be seen). A CV ≈ 1.0 would probably have been more appropriate to the analysis than was a CV ≤ 0.4. At this level of variation truncation at 0 would have a major effect on simulation results, and use of the mode rather than the mean (as was used in the paper) would be important to producing valid results.

It is very unlikely that the models presented in Boyce et al. (1998) account for major changes in foraging behavior and related sightability of bears that occurred in the Yellowstone ecosystem, especially between 1986 and 1992. During this time, grizzly bears in the eastern part of the ecosystem increasingly engaged in the excavation of army cutworm moths from alpine talus, largely between the dates of 15 July and 15 September (Mattson 1991; French *et al.* 1994). Coincident with this trend, an increasing fraction of all the unduplicated females with COY tallied during a given year were first seen on a moth site (Mattson 1997). In 1986, no females with COY were first seen on moth sites, whereas by 1992 fully ⅔ of such "unique" individuals were first seen on such sites. At the same time, virtually all of the absolute increase in sightings of females with COY during this time was attributable to aerial observers seeing bears on moth sites (Mattson 1997). In light of the fact that females foraging on moth sites are uniquely detectable (≈ 1) (*cf.* O'Brien & Lindzey 1998), there is good reason to expect that there have been major trends in detectability of females with COY over time. Effort by aerial observers has also systematically increased. As a bottom line, although Boyce et al. (1998) present interesting theoretical work, **they do not provide sufficient grounds, either by more stringent scientific standards or by the weight of evidence, for rejecting the hypothesis that annual variation in their estimates of total population size is substantially affected by variation in detectability of females with COY and by variation in search effort by human observers** (Mattson 1997).

Given these considerations, **your conclusions regarding (i) rate of population growth for the Yellowstone population and (ii) the soundness of using counts of females with COY for monitoring trend and calculating allowable mortality are not tenable.**

2. Prescription and Application of Methods for Management

The 1993 Grizzly Bear Recovery Plan and the recent document presenting "Further information..." are ostensibly exercises in policy invocation, application and prescription. As such, these activities are subject to standards. In other words, not all invocation, application and prescription of policy is performed equally well. Lasswell (1971) provides standards by which these activities related to grizzly bear recovery can be judged: **prescription** should be *open* and *comprehensive*; **application** should *build consensus and cooperation* and *serve the common interest*; and **invocation** should be *open*, *dependable*, and *not subject to abuse by those involved in the sanctioned process*. An implicitly important part of all these activities is the **specification**

of risk associated with the proposed action, both to the managed resource (in this case grizzly bears) and to interest groups. In light of these considerations I have some additional comments:

Comprehensiveness. — **Your supplemental information is not sufficiently comprehensive.** A number of relevant papers as well as relevant information presented at fora that you attended were not considered. For example:

(i) Dr. Craig Pease and I have presented our concerns about existing analyses, along with our methods and preliminary results for a demographic analysis of Yellowstone's grizzly bear population, at several well-attended scientific fora, including the 10th International Conference on Bear Research and Management held in Fairbanks, Alaska and the Carnivore Conference held in Mammoth, Wyoming, both in 1995. Our expressed concerns date back to the public-comment period on the 1993 Grizzly Bear Recovery Plan.

(ii) I also presented a paper, currently in press (Mattson 1998), at the 10th International Conference. This paper is directly relevant to prescribing the way that mortality counts are used for monitoring grizzly bear populations in the contiguous United States.

(iii) I also had a paper published in the journal *Biological Conservation* (Mattson 1997), available at the time that "Further information..." was written, that is directly relevant to how counts of unduplicated females with COY are used to calculate allowable mortality.

(iv) Dr. Dan Doake published a paper in *Conservation Biology* (Doake 1995) concerning the limits of inference about population status based on analysis of trends using historical demographic data. He used grizzly bears and the Yellowstone ecosystem as exemplars.

(v) I and a number of co-authors have published several papers and several book chapters in recent years that extensively discuss issues directly relevant to recovery-related prescription of approaches and methods for Yellowstone's grizzly bears (Mattson & Craighead 1994; Mattson 1996; Mattson *et al.* 1996*a*, 1996*b*).

(vi) Dr. John Craighead and his associates published a major scientific work on Yellowstone's grizzly bears (Craighead *et al.* 1995) in which demographic estimates and methods were extensively discussed.

None of these papers or associated information is presented or discussed in the "Further information..." document.

Openness. — **The process of sanctioned and officially-funded scientific research, as well as the development of methods for monitoring grizzly bear populations in the contiguous United States, have not been open.** The selection or exclusion of participants has not been independent of their views of methods and metrics adopted by the 1993 Recovery Plan. Sanctioned participation has been carefully restricted to those initially supportive of Recovery Plan methods; subsequent involvement of previously uninvolved scientists has occurred in ways apparently designed to carefully control and shape their perceptions. Other individuals, known to be critical of approaches presented in the 1993 Recovery Plan have been systematically and almost totally excluded. The lack of comprehensiveness pointed out above is symptomatic of this approach. The willingness to invoke unpublished technical reports supportive of Recovery Plan methods while ignoring similar unpublished work that is more critical, is also symptomatic.

Specification and management of risk. — All methods involve some degree of uncertainty. A certain amount of risk is incurred by different interest groups and by the managed biota depending on how this uncertainty is managed or whether it is even disclosed. Management of uncertainty involves, first, disclosing its nature and extent. Secondly, such management involves appraising the consequences of "the world" actually exhibiting all of the possible states within the identified range of uncertainty. For example, these consequences can include the likelihood of the population increasing when we think it is decreasing, or visa versa. **The "Further information..." document does not contain any meaningful disclosure and appraisal of uncertainty and associated risk.** The few appraisals of uncertainty and risk drawn upon by the 1993 Recovery Plan and the "Further information..." document have been deficient in important ways:

(i) As pointed our earlier, the range of variation in sighting probabilities or rates considered by Boyce et al. (1998) was very likely too limited. Furthermore, these authors did not explicitly appraise the risks of using their estimates of total population size to monitor trend or calculate allowable mortality (although they did interject a cautionary statement).

(ii) It is consistently claimed that the 4% rate used to calculate allowable mortality is conservative (*e.g.*, Page 37). Mattson (1997) explains why this is not the case. In addition, the stated rationale put forth for the 4% rate being conservative rests on the assumption that ~⅔ of all mortality is documented. This assumption is arbitrary (and somewhat irrelevant [Mattson 1997]). The best current information (which is scientifically very weak) suggests that < ½ of all mortality is documented. Recent, and more reliable, calculations by Dr. Pease and myself suggest that this fraction is much lower.

> As an aside, there is some potentially misleading information presented in the first paragraph of page 37. You claim that mortality limits (4% of minimum calculated population size) would not have been exceeded in recent years if recorded mortality had been compared to an estimate of total population size rather than minimum population size. This would be comparing apples and oranges. Recorded mortality is actually a minimum figure and is more appropriately compared to a minimum estimate of population size. Even so, depending on the ratio of these two minimums to their respective totals, there are major problems even with this comparison (Mattson 1997).

(iii) None of the standards adopted by the 1993 Recovery Plan have been explicitly related to the probability that the grizzly bear population will persist some specified time into the future. Without being explicitly related to some such prognosis, the adopted standards run the risk of being arbitrary. This is clearly the case with the standard for distribution of sightings of females with young. There is no rationale for the adopted standard other than an unconvincing appeal to first principles; there is also no specification of what constitutes "adequacy" (cf. Page 33). Validation of the standards continues to rest on problematic appeals to unreliable circumstantial evidence (as discussed above; see Pages 37 and 46).

This assessment needs to be clearly distinguished from the work on population viability by Dr. Mark Boyce (Boyce 1995). This work relies solely on demographic data. It also used an estimate of population growth rate that was biased high. There was no explicit tie to the invocation and application of standards prescribed in the 1993 Recovery Plan. Dr. Boyce also concludes that any meaningful analysis of population viability, hitherto not yet done, depends on some tie to explanatory habitat features.

As a bottom line, **even though the "Further information..." document is an official endeavor ostensibly devoted to the invocation and prescription of policy, it more closely resembles promotion** (*sensu* Lasswell 1971). This is very clear in the tone regarding monitoring methods, excluding the text penned by Dr. Boyce. **This distortion of the process predictably results from a lack of openness and comprehensiveness, and has resulted in or been associated with a degeneration of cooperation and consensus.**

3. Recommendations

(1) Researchers with criticisms or concerns about methods adopted by the 1993 Recovery Plan and promoted in the "Further information..." document should be explicitly engaged in the sanctioned process by which these methods are developed and appraised.

(2) Rather than funding researchers on the basis of sole-source contracts, government funding should be allocated on the basis of competition, with review of proposals by independent scientific panels.

(3) Demographic data from the Yellowstone grizzly bear population should be freely available to other scientists for independent demographic analyses. This is critical to alleviating the historically closed and monopolistic situation perpetrated by sanctioned government scientists.

(4) All relevant information and papers should be objectively considered and analyzed in prescribing and appraising methods for judging recovery of grizzly bear populations. Such considerations and analysis should be fully disclosed.

(5) Expert help should be sought for design of a risk analysis related to recovery methods and criteria. Results of such an analysis should be fully disclosed and part of the basis for choosing methods and criteria.

(6) The "Further information..." document and parts of the Grizzly Bear Recovery Plan should be recast and rewritten so as to be consistent with the invocation, application and prescription of policy rather than, as is currently the case, with the promotion of certain methods.

(7) On-going work on a habitat-based population viability analysis for the Yellowstone grizzly bear population, by Doug Ouren and Dr. Mark Boyce, should be explicitly integrated with on-going habitat-related research of other scientists, including myself, working with Yellowstone grizzly bear data.

(8) The models presented in Boyce et al. (1998) should be expanded to better accommodate annual heterogeneity in detectability of females with COY. At the very least, a greater range of heterogeneity should be considered in simulations.

(9) Means for estimating actual levels of mortality should be developed. As I indicated earlier, much of this work has already been thought out by scientists not involved in officially-sanctioned science.

(10) The most defensible and least biased estimates of population growth rate should be adopted until better estimates are produced. This is preferable to continually invoking less defensible estimates contained in unreviewed technical reports. As I indicated earlier, a more defensible estimate of population growth rate has been presented at scientific fora and is contained in a paper in review and likely to be published within the next year.

(11) The consequences, risks, and uncertainties of a *range* of criteria should be investigated using spatially- and temporally-explicit estimates of detectability for females with COY. Existing models developed by Dr. Craig Pease and myself, with some extension, are potentially capable of such an analysis.

(12) Methods for calculating allowable mortality should reflect already identified sources of risk and uncertainty and employ (i) a lower mortality rate (i.e., <4%), (ii) some rationale for using a confidence interval bound or mean for estimates of reproductive interval and proportion of adult females in the population.

These are just a few of potentially many changes that could improve the scientific basis for grizzly bear recovery and the application of this science to development of recovery criteria and methods.

Literature Cited

Boyce, M.S. (1995). Population viability analysis for grizzly bears: a critical review. Report to the Interagency Grizzly Bear Committee, Missoula, MT.

Boyce, M.S., Manly, B.F.J., Cherry, S., Knight, R.R., & Servheen, C.W. (1998). Cumulative counts of unique individuals for estimating population size. *Journal of Wildlife Management* 62: In press.

Craighead, J.J., Sumner, J.S., & Mitchell, J.A. (1995). *The grizzly bears of Yellowstone: their ecology in the Yellowstone ecosystem, 1959-1992.* Island Press, Washington, D.C.

Doak, D.F. (1995). Source-sink models and the problem of habitat degredation: general models and applications to the Yellowstone grizzly. *Conservation Biology* 9: 1370-1379.

Eberhardt, L.L. (1995). Population trend estimates from reproductive and survival data. Pages 13-19 *in* R.R. Knight & B.M. Blanchard. Yellowstone grizzly bear investigations: report of the interagency study team. U.S.D.I. National Biological Service, Bozeman, MT.

Eberhardt, L.L., Blanchard, B.M., & Knight, R.R. (1994). Population trend of the Yellowstone grizzly bear as estimated from reproductive and survival rates. *Canadian Journal of Zoology* **72**: 360-363.

French, S.P., French, M.G., & Knight, R.R. (1994). Grizzly bear use of army cutworm moths in the Yellowstone ecosystem. *International Conference on Bear Research & Management* **9**: 389-399.

Hovey, F.W. & McLellan, B.N. (1996). Estimating population growth of grizzly bears from the Flathead River drainage using computer simulations of reproduction and survival rate. *Canadian Journal of Zoology* **74**: 1409-1416.

Lasswell, H.D. (1971). *Pre-view to the policy sciences*. Elsevier, New York.

Mattson, D.J., Gillin, C.M., Benson, S. A. & Knight, R.R. (1991). Bear feeding activity at alpine insect aggregation sites in the Yellowstone ecosystem. *Canadian Journal of Zoology* **69**: 2430-2435.

Mattson, D.J., Blanchard, B.M. & Knight, R.R. (1992). Yellowstone grizzly bear mortality, human habituation, and whitebark pine seed crops. *Journal of Wildlife Management* **56**: 432-442.

Mattson, D.J. & Craighead, J.J. (1994). The Yellowstone grizzly bear recovery program: uncertain information, uncertainty policy. Pages 101-129 *in* T.W. Clark, R.P. Reading & A. Clarke, editors. *Endangered species recovery: finding the lessons, improving the process*. Island Press, Washington, D.C. (Invited book chapter).

Mattson, D.J. (1996). Ethics and science in natural resource agencies. *BioScience* **46**: 767-771.

Mattson, D.J., Herrero, S., Wright, R.G. & Pease, C.M. (1996*a*). Science and management of Rocky Mountain grizzly bears. *Conservation Biology* **10**: 1013-1025.

Mattson, D. J., Herrero, S., Wright, R.G. & Pease, C.M. (1996*b*). Designing and managing protected areas for grizzly bears: how much is enough? Pages 133-164 *in* R.G. Wright, editor. *National Parks and protected areas: their role in environmental protection.* Blackwell Science, Cambridge, MA. (Invited book chapter).

Mattson, D.J. (1997). Sustainable grizzly bear mortality calculated from counts of females with cubs-of-the-year: an evaluation. *Biological Conservation* **81**: 103-111.

Mattson, D.J. (1998). Changes in the mortality of Yellowstone's grizzly bears. *International Conference on Bear Research & Management* **10**: In press.

O'Brien, S.L. & Lindzey, F.G. 1998. Aerial sightability and classification of grizzly bears at moth aggregation sites in the Absaroka Mountains, Wyoming. *International Conference on Bear Research & Management* **10**: In press.

U.S. Fish & Wildlife Service (1993). Grizzly bear recovery plan. U.S. Fish & Wildlife Service, Missoula, MT.

If you have any questions or would like to engage in a more free-ranging discussion of any issues that I have raised, please call me. If you would like any of the referenced papers, I would be more than happy to send you those that are available to me. In other words, I would be glad to be of much help as I can in promoting good science and process.

Sincerely,

David J. Mattson
Research Wildlife Biologist