University of Idaho
College of Natural Resources
Department of Fish and Wildlife Resources
P.O. Box 441136
Moscow, Idaho 83844-1136
Phone: 208-885-6434
Fax: 208-885-9080
E-mail: fish_wildlife@uidaho.edu
Web: www.its.uidaho.edu/fishwild

September 2, 2003

Memorandum

To: Terese Johnson, Wildlife Biologist, Rocky Mountain National Park
cc: Ryan Monello, Terry Terrell, Kathy Tonnessen, Linda Whitson

From: Gerry Wright, USGS Unit Scientist, Idaho Cooperative Fish and Wildlife Research Unit

Subject: Peer Review of draft ms: "The elk herd in Rocky Mountain National Park"

Enclosed are two peer reviews of the above referenced document. I also enclose my own comments on the manuscript. Also enclosed are the edited copies of the draft report. The peer reviews were funded under Task Agreement No. UID-02, through the Rocky Mountains CESU and constitute a final report of that agreement.

For background the peer reviews were provided by : 1) Dr. Douglas Houston. Dr. Houston is a retired NPS Research Wildlife Biologist, renowned for his expertise in elk management and ecology in national parks. Throughout much of his early career he was the chief researcher studying elk in Yellowstone National Park. Later in his career he concentrated on Roosevelt elk and other ungulates in Olympic National Park. 2) Dr. James M. Peek. Dr. Peek is a professor-emeritus at the University of Idaho. Most of his career was spent doing ungulate research in the Rocky Mountains and Alaska, and teaching at the U of I. Both reviewers were 'present at the birth' of some of the significant changes in the management of ungulates in national parks that occurred in Yellowstone in the late 1960s and early 1970s. Rocky Mountain N. P. is fortunate to have the guidance in terms of their reviews provided by these two acknowledged experts in the field of elk management.

Both reviews acknowledged the fact that the manuscript was generally well written and readable. In addition to written comments, Dr. Houston has provided extensive written editorial and clarification comments directly on the report. The written comments of both reviews are quite detailed, below I summarize the salient points of each reviewer.

Dr. Peek:

This review criticizes the definition applied to the term 'natural regulation' implying that it is too narrowly defined. It makes the case that unlike Yellowstone, under current conditions at ROMO, i.e., no major predators, the concept of natural regulation cannot be fully evaluated in all its ramifications. The review provides valuable and unique insights into the history of the NPS formulation of natural regulation policies and what is implied by the term "natural". He discusses the fact that the natural regulation management program for Yellowstone was originally set up as a hypothesis to be modified as more knowledge was obtained. This approach is now commonly termed adaptive management.

The review criticizes the general organization of the paper, pointing out that there is a lot of redundancy among the different sections. The same issues are covered in several different places disrupting the flow of the paper. The reviewer provides several suggestions for reorganization along topical lines.

The review makes the point that all of the research in the park to date has been short-term. To answer the fundamental questions that are being asked, particularly whether elk numbers are food-limited and/or predation limited will require long-term data. Thus the reviewer feels that some judgements made in the paper may be valid but may be premature.

Dr. Houston:

The review points out that much more background information describing the winter range vegetation in the park is needed to fully understand the situation and this information should be presented in the beginning part of the paper. The reviewer suggests that the manuscript more fully describe the interactive nature of ungulate grazing systems, that the elimination of native ungulates that occurred in the region in the early years should have had profound changes in the vegetation as would their reconolonzation of the same range at a later point in time. This would occur irrespective of the confounding effects of livestock grazing and fire suppression.

The review urges the manuscript refrain from the use of terms like 'overgrazing' and 'deteriorated forage conditions' as they are not clearly defined and represent in many people's eyes a bias in interpretation. The review asks for better clarification of the various models, including the assumptions made on parameters and whether linear relationships were assumed. The review strongly suggests deleting the section showing comparative estimates of elk densities for other park areas in the Rocky Mountains as the situations and sampling periods in those areas differ greatly from ROMO.

The review suggests that the authors look at the decline in willows in a broader context, that is it is occurring region wide, and is probably influenced by complex driving forces, many of which are anthropogenic. It urges that the report reconsider its comments with respect to nutrient cycling, and suggests that the findings in Yellowstone may well be applicable to ROMO. Finally, the review considers the conclusions to be overstated as many of the caveats presented in the text were removed. Alternative wording for conclusion 1 is suggested.

Dr. Wright:

I would add only one additional comment to those stated by the peer reviewers. Although it may seem strange coming from a former modeler, I was somewhat disturbed by the reliance throughout, on the output of the Savannah model. I know this model has long been under development and the park has invested a lot of time and funding in its development. The reputation of the model developers is first rate. I also realize that a heavy reliance on the model simulations was necessary because of a lack of historical data. However, given the emphasis the model received, more detail about the assumptions made in the model, and the data used to develop it should be presented. Some sensitivity analyses to see how the model responds to variations in certain key parameters would also be appropriate. I am also concerned on the reliance on model results when, to my knowledge, neither the model nor its outputs, have not been through any type of peer review, i.e., journal publication.