United States Department of the Interior

FISH AND WILDLIFE SERVICE
7920 NW 71st Street
Gainesville, FL 32653

September 6, 2002

Mr. Scott Sanders
Florida Fish and Wildlife Conservation Commission
620 South Meridian Street
Tallahassee, FL 32399-1600

Dear Scott:

As requested, I have reviewed the three vegetation studies entitled *A Comparision of Grazed and Ungrazed Plant Communities in Four Peninsular Florida Wildlife Management Areas*, *Effects of Various Management Regimes on Vegetation in Mesic and Scrubby Flatwoods Communities*, and *Efficacy of Various Management Regimes on Restoring Long-unburned Scrub to an Early Successional Structure Favored by Obligate Scrub Species*. Because minimal guidance was provided to me as a reviewer, I took the liberty of evaluating the studies in three basic areas: (1) conceptualization (e.g., relevance, originality, coherence, reliance on previous studies, etc.); (2) study design, as well as data collection and analysis; and (3) report preparation. I also consolidated my comments because I found that most were applicable to all three studies. Finally, you should be aware that I did not concern myself with comments of an editorial nature.

Let me start by saying that I was generally impressed with the studies as described in the documentation provided, and certainly found no critical errors in reasoning or execution of the studies. Overall, I would give a grade of "B" in terms of conceptualization, and an "A" in terms of both design/analyses and report preparation. My more specific comments below should not be construed as any sort of serious criticism of the studies in question, but are intended primarily as considerations for the design of similar studies in the future. These comments generally fall into four categories (in no particular order): (1) goals and objectives; (2) the issue of scale; (3) the utility of null-hypothesis testing; and (4) detection probabilities. I recognize that some of these issues may have been discussed and resolved elsewhere in development of the studies, so feel free to disregard my comments as appropriate.

Goals and objectives.–As I understand it, the motivation for these studies was the need either to justify a current management practice (in the case of grazing) or to develop the most effective management regime (in the case of flatwoods and scrub communities). The principal obstacle to meeting these goals was perceived to be uncertainty concerning vegetation responses to various management treatments. However, it was not always clear how a reduction in that uncertainty might be used to justify or modify management practices, presumably because there is collective ambiguity about how the specified vegetation attributes (e.g., shrub height) relate to the goals of restoring/enhancing/protecting community diversity, native plant communities, or ecosystems. This

problem is manifested in the Comprehensive Management Plans for the study areas, in which broad, qualitative conservation goals are articulated, but then accompanied by objectives that are statements of desired actions rather than of desired system attributes. I appreciate the fact that you and others in FWC have recognized this problem (and are attempting to address it), but couldn't help pointing it out here because of the important implications for these sorts of studies. Ideally, the desired system attributes (i.e., important indicator variables like shrub height and their range of desired values) need to be specified *a prior* so that plant monitoring can be designed and implemented most efficiently. I appreciate the difficulty of this task, however, and think the investigators probably did a good job of identifying those vegetative attributes that were likely to be of most interest.

My second comment regarding goals has to do with the notion of adaptive resource management. In spite of rather loose definitions that currently are popular, the original focus of adaptive management was the so-called dual-control problem. The dual-control problem is a manifestation of the fact that the manager actually controls (within limits) both the ecological system itself *and*, through his/her actions, the ability to understand the dynamics of that ecosystem (and, thus, the ability to reliably predict management effects). So on the one hand, the manager can pursue traditional management goals within the constraints imposed by his/her limited understanding of system responses, or the manager can try various management actions in some randomized fashion so as to learn effectively about system responses. Of course, the manager also could do these things simultaneously in different places (or at different times), which is basically what is being done in these three studies. This approach is not optimal, however, because management does not directly benefit from the research while it is ongoing, and because there is no guarantee that what is learned at the sites (or times) chosen for research will be universally applicable (although the studies in question did randomize the location of the research, they did not randomize over time). The ultimate challenge, then, is to integrate management and research across space and time in such a way that short-term management returns in the face of uncertainty are balanced with the long-term value of reducing that uncertainty. This is not a trivial optimization problem, but Carl Walters has some helpful thoughts about it:

Walters, C. J., and R. Hilborn. 1978. Ecological optimization and adaptive management. Ann. Rev. Ecol. Syst. 9:157-188.

Walters, C. J., and C. S. Holling. 1990. Large-scale management experiments and learning by doing. Ecol. 71:2060-2068.

Walters, C. J., and R. Green. 1997. Valuation of experimental management options for ecological systems. J. Wildl. Manage. 61:987-1006.

and it's the approach that my colleagues and I were pursuing at the Goodwin Waterfowl Management Area (http://www.fcsc.usgs.gov/SEAMG/seamg_products/BayesExampleRev.pdf). In any case, the dual-control problem seems to have rather profound implications for the way in which research in support of management is conducted.

The issue of scale.–Ecological systems exhibit variation on a continuous range of spatial, temporal, and organizational scales, ultimately as a function of how individuals respond to (and in turn influence) the environment. Ecological research and management must be conducted within

artificially imposed spatial, temporal, and organizational bounds, but often these bounds are somewhat arbitrary and the focus on system dynamics rarely extends beyond these bounds. Examples in management include annual application of treatments, designation of forest stands within a management area, and selective harvesting of a single species. Traditional ecological research suffers from the same myopia, and is usually conducted on the small spatial and temporal scales that are logistically convenient. But it is increasingly clear that effective ecological management and research requires accounting for system dynamics at multiple scales, as well as how phenomena (system attributes and processes) at a particular scale are influenced by dynamics at smaller/faster and larger/longer scales. This fact has some important implications for the design of linked research-management programs.

Let me try to give a couple of examples based on the three studies in question. First, let me define a bound on a particular scale as consisting of a "grain" and an "extent." The grain is the smallest, indivisible unit of observation (e.g., a 1 m^2 vegetation sampling quadrat, or 1 year in the case of annual sampling). There will be variation in system attributes within the grain, but this variation is averaged out at the level of the grain and, thus, is not observable. The extent is simply the sampling universe to which sample-based inference applies (e.g., the management unit or stand). The grain and extent then form a "window" through which management is applied and responses observed. As I understand it, the grain and extent, respectively, for the scales of the three studies are: (1) temporal - 1 year and 10 years; (2) spatial - quadrat and stand; and (3) organizational - species and community (or vegetation "type"). Potentially important questions to ask are: What role does variation in seasonal dynamics play in determining system dynamics and management responses within the window of observation? Similarly, where does a 10-year period fit within the pattern of variation on longer time scales, and can this particular 10-year response be expected to a reliable predictor of responses in any other 10-year period (i.e., the so-called problem of transient response)? How do species associations within quadrats affect vegetation structure within the level of the stand? How does the layout of stands at larger scales affect ecological attributes (like the presence or persistence of rare species) within stands? The current study designs often account for these extraneous sources of variation in order to strengthen inference about a treatment effect, but they are regarded primarily as a nuisance rather than a subject of interest per se.

Obviously, the problem of scale presents great intellectual and logistical challenges to study design, but a recognition of the problem is the first step to a solution. In that regard, I found the explicit description of scale issues in the Introduction of *Efficacy of Various Management Regimes on Restoring Long-unburned Scrub to an Early Successional Structure Favored by Obligate Scrub Species* a good first step.

The utility of null-hypothesis testing.–I wanted to say something about this mainly because of the controversy articulated in recent articles of the Journal of Wildlife Management and Wildlife Society Bulletin. Those opposed to null-hypothesis testing argue that variation is a pervasive feature of ecological systems and, therefore, null-hypothesis testing is a trivial exercise (a failure to reject the null hypothesis tells you nothing other than your sample size was not big enough). While I have a good bit of sympathy for this position, I also wanted to applaud the investigators for explicitly stating the magnitude of differences considered "interesting," and for careful consideration of sample sizes necessary to detect those differences at a reasonable level of confidence. Of course, the real difficulty lies in defining the criteria with which to determine the magnitude of "interesting

differences." Those criteria ultimately must be linked back to the goals and objectives of management in some explicit way. But given the considerable ambiguity about those goals and objectives, I thought the investigators did a good job of providing an important context for the hypothesis testing that is such a prominent feature of these studies.

Detection probabilities.–I don't intend to say much about this because I could quickly get beyond the bounds of my limited statistical expertise. But have you considered the possibility that the probability of detecting a particular plant species in a sample quadrat is <1 or that it varies among species or among observers? Of course, there are a number of ways in which this could be tested (e.g., double observers on a sub-sample of quadrats) and the bias (if it exists) removed. It might be something to think about.

That's it. In general, the studies seem to be well planned and executed and I have no serious criticisms (to the extent of my ability to identify them). I think more of these types of efforts are warranted, perhaps with a more explicit recognition of some of the issues I've discussed. In any case, these studies help provide a foundation for a more adaptive approach to management by tying study design back to management goals and objectives, by identifying a finite set of management treatments thought most likely to be successful, by developing managerial and logistical support needed for field experimentation and system monitoring, and by providing information that can be used to develop predictive models of vegetation dynamics. These are significant accomplishments.

I enjoyed reviewing these studies and hope that at least some of my comments are useful. Please fell free to call upon me if you have any questions about my comments.

Sincerely,

Fred Johnson
Wildlife Management Biologist