Review of "Evaluating key uncertainties regarding road grooming and bison movements," Robert A. Garrott and P. J. White, by Joshua A. Millspaugh.

Note to readers:
Dr. Millspaugh is Associate Professor of Wildlife Conservation in the Department of Fisheries and Wildlife Sciences, School of Natural Resources, at the University of Missouri-Columbia, and the **Director** of the Thomas S. Baskett Wildlife Research and Education Center at the same institution. He has a Ph.D. in wildlife ecology from the University of Washington at Seattle. The recipient of numerous grants for wildlife ecology research and monitoring with a number of different state and federal agencies, Dr. Millspaugh has published extensively on the same subjects. His full CV is available by e-mailing yell_winter_use@nps.gov.

Dr. Millspaugh's review follows.

Below are specific comments on the proposal by theme. The proposal represents a mix of very well-thought out hypotheses, experiments, data mining, and several studies described at levels ranging from very detailed to very general. I believe there is a very high probability that these studies will reduce the uncertainties associated with key aspects of bison management at Yellowstone National Park. Garrott and White have extensive experience in this system and have an excellent track record of producing high quality papers.

Theme 1: Influence of snow and terrain on bison movements

The authors do a great job of laying out detailed predictions based on the hypothesis that snow depth, a critical threshold for foraging, drives winter movements. Specific comments are below.

1. I wondered whether this was the only plausible hypothesis? This research might be more powerful if a plausible contrasting (or partially contrasting – an interaction of snow and some other factors) hypothesis/hypotheses were established as viable candidates.

2. There is a general concern about the use vs. random analysis given comments by Keating and Cherry (2004). Have you considered alternatives such as case control designs (see comments below)?

3. I was curious about the average error of the snow model estimates. Error must be pretty low and unbiased for this application. How will error be accounted for when model output is used as a logistic-regression covariate?

4. The thresholds might be estimated a bit coarsely. My general experience with logistic regression output indicates that confidence intervals around what should be ecologically tight relationships often are very big. This might have implications to your study. Also, although the approach will account for major bison trailing effects, it may not be

sensitive to partial trailing in local patches, resulting in some homogenizing between used and random patches. Finally, bison for this component seem to be in areas with groomed travel routes. Less energy spent traveling = more energy available for foraging, and a higher threshold compared to if groomed travel routes were not available.

5. What is the justification for a 99% kernel and an 1850 m bandwidth? It was unclear why random data from a 99% probability contour would be used to look at fine-scale relationships. You might consider a discrete-choice approach, with random locations within some radius of each observed location. This seems necessary in addition to or in place of this coarse approach. I am concerned that the 99% contour is poorly defined with kernel estimators, and you are measuring random points at locations far from what are realistically available to the individual. Will each individual be analyzed as a sample unit, or will observations be pooled? Would random points be limited to similar terrain as that of used locations, to avoid confounding snow depth with other factors?

The bandwidth selection of 1850 m seems arbitrary and this *a priori* selection should be explained. You might look at the paper by Gitzen et al. (2006) for some recent thoughts about bandwidth selection in kernel analysis.

Gitzen, R. A., J. J. Millspaugh, and B. J. Kernohan. 2006. Bandwidth selection for fixed kernel analysis of animal range use. Journal of Wildlife Management 70:1334-1344.

6. I am not fully convinced of the proposed approach (p. 18) of fitting only linear covariate forms in the initial AIC-based model ranking, and then in this initially high-ranked model set, incrementally testing each non-linear form of the covariate. Particularly since existing analyses (Bruggeman et al. 2006) could be used to guide this analysis. So much of the analysis up to this point is based on careful *a priori* thought, which is great. I would suggest you stick with that approach. I suggest doing the same with the functional-covariate relationships: chose which (maybe all) of these forms make the most sense biologically and include *a priori* models that differ in this functional form. Explore other relationships post hoc but keep them out of the model set. The power of information-theoretic approaches comes from pre-specifying all meaningful models. I do not see the merit of putting the functional exploration in a gray area between *a priori* and post hoc.

7. My general opinion is that the logistic-regression aspects of this proposal are interesting, and will produce useful results. However, I am curious about whether snow-movement relationships will be estimated well enough to provide precise answers to the underlying questions (p. 8-9). With such a large number of GPS-observations for some components of this theme, the authors might consider going beyond standard logistic-regression resource selection analyses, and make use of the time series of observations. For example, time spent in some patches/locations, and travel vectors between locations (I did not have access to Bruggeman's Ecological Applications paper which is "in press" to determine whether they took such an approach). It might be appropriate to make full

use of the sequence of observations, preferably via mechanistic models of the movement process.

Theme 2: Determining drivers of migration, re-distribution, and demographic characteristics

This section describes additional analyses that will be performed for several existing, partially overlapping data sets. I had some difficulty determining which planned analyses are completely new, which are significant re-analyses, and which are relatively minor updates (with some new data) of strong completed analyses. For example, the authors propose a study of bison migration dynamics at a large scale, then describe conclusive results from a finer-scale study, but do not identify how they will integrate this past fine-scale study with proposed large-scale analysis. As a result, it was unclear how the proposed demographic analyses (pp. 27-29) would provide more precise insight towards solving the management problem at hand, than any of the many demographic publications cited (including many by the proposal authors). A few sentences might help clarify these issues.

The management question justifying this study (Themes 1 and 2) seems clear: In the absence of road grooming, would bison still migrate to the Northern Range, and would over-winter survival, density, and out-of-park movements change?

Theme 3: Effects of road grooming on bison use of travel corridors

I realize that experimental road closures are highly contentious at YNP. It seems clear that such adaptive-management experiments are the only way the most critical management-related question (how bison would respond to no grooming) can be answered definitively, at least in terms of the short-term behavioral responses. This theme (at least the experimental aspects) is by far the most relevant and necessary part of the proposal.

1. Certainly, with GPS collars on 50-60 bison, a wealth of data will be collected, and many interesting analyses could be performed. There was a lack of detail regarding analysis here compared with other sections of the proposal. Some more detail in this regard would be helpful.

2. Regarding deployment of camera systems. The authors state that previous systems have had problems with power supply in extreme cold, as well as other factors. They might bolster the case for the likelihood that their new system will overcome these problems – it reads a bit exploratory. Also, I am not able to tell how data-rich the images will be – whether there is much chance of capturing data sufficient for interpreting behavior and number of bison traveling together. In Bjornlie and Garrott (2001), only the number of bison photo-events and the direction of travel were discussed.

3. Firehole Canyon Manipulation (and the two subsequent manipulations). This seems like a clever experiment, and the authors are very realistic about potential issues with the

experiment. As they state, this experiment may take several winters to implement successfully (i.e., kinks worked out, sufficient snowfall).

4. I do not have a good feel for whether conclusions from this experiment will apply widely throughout the park – whether snowfall amounts, landscape pattern, etc., are similar enough here to the rest of the groomed roads in question so that this single experiment will give strong insights into effects of grooming in other areas.

5. Given that the proposed camera system seems so early in development, I wonder whether the authors should consider a more reliable primary way of detecting how bison respond to the closure – direct observation of the gate and/or daily snow-trail checks? I would want to ensure they collect relevant data if the camera system fails.

6. Will you integrate results from the three planned manipulations into a single analysis?

7. The logistic regression analysis (1= used ungroomed, 2 = turned back) assumes each group is independent. However, I'm not convinced that is the case. If one group uses the ungroomed road, subsequent groups are more likely to use the road (and its packed trail). If a group turns back once and the same individuals come to the gate again, they may be more likely to give up, or more desperate to find an alternate route. How will you account for this non-independence?

Last comment

The work being proposed is solid. I have one last suggestion that the authors might consider. In my view, this suggestion would not need to be incorporated in a final proposal.

In the absence of strong, multi-year road-closure experiments – long-enough to assess consequences for demography and outside-of-park movements – you could combine the smaller-scale closure experiments (Theme 3) with a demographic/landscape-movement simulation-modeling approach to specifically examine the underlying questions. This model could be used to integrate all the relevant information discussed in Themes 1 and 2. Construct a demographic spatially explicit model that relates survival and outside-of-park movements to snow levels and the ability to migrate or move on groomed roads. You could follow the guidelines of Starfield – start simple, add in complexity as necessary, and identify which supplementary studies of Themes 1 and 2 really are essential for building/testing the model. This approach might result in a more problem-driven approach that might prove powerful.