Southern Alberta Landscapes:
Meeting the Challenges Ahead

External Review of ALCES Sector Models

Prepared for
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Executive Summary

The Miistakis Institute was contracted by Alberta Environment to coordinate an external review of modules developed for use in ALCES® (A Landscape Cumulative Effects Simulator) modelling in the Southern Alberta Sustainability Strategy (SASS) planning area. The review of 4 sub-models, which were prepared specifically for the SASS, included:

- Wildlife and Biodiversity,
- Invasion of Non-native Plant Species,
- Natural Disturbance (fire and insects), and
- Rangeland Plant Community Structure.

Potential reviewers were identified by Alberta Environment and the Miistakis Institute with the final selection made by the Miistakis Institute..

The selection of reviewers was based on a set of criteria provided by Alberta Environment. The criteria indicated the selection of experts in the respective subject area with neither any vested interest in the outcome of the review nor any direct affiliation with the authors of the modules or the sponsoring agency. Given these criteria, university-based experts from Alberta constituted 4 of the 5 reviewers with the 5th coming from a federal research station:

- Edward Bork, University of Alberta (Rangeland Plant Community Structure Dynamics),
- Mark Hebblewhite, University of Alberta, (Wildlife and Biodiversity),
- Barry Irving, University of Alberta (Rangeland Plant Community Structure Dynamics),
- Edward A. Johnson, University of Calgary (Natural Disturbance),
- Walter Willms, Lethbridge Research Centre, Agriculture and Agri-Food Canada (Invasion of Non-native Plant Species).

Reviewers were asked to critically assess the sub-model documentation guided by the following five questions:

- Is the approach/methods used sound?
- Are assumptions valid and/or justifiable in light of the information available?
- Are data used in an appropriate way?
- Does the module address critical aspects of the module subject area?
- Does the module address critical issues for strategic planning in southern Alberta?

The rangeland structural dynamics sub-model was reviewed by two rangeland specialists. Both reviews indicated that the model was appropriate for strategic-level planning and that the approach was sound. Many of the issues raised by the reviewers reflected a need for better explanation for derivation of model parameters. The reviewers both raised questions regarding model assumptions and the details of using specific parameters. Reviewers expressed the concern that the sub-model may be over-general if were to be applied for predictive purposes in site-specific, predictive cases.

The review of the wildlife and biodiversity sub-model questioned the title of the module and recommended that 'biodiversity' be dropped for accuracy sake. The reviewer recognized the strengths in the Delphi (expert) approach in the selection of focal species, but questioned its strength in the development of the habitat suitability index (HSI) models. In addition, the reviewer points out the failure to incorporate measures of uncertainty into the models. The quantitative HIS and validation approach applied to the grizzly bear was deemed sound and appropriate, but the reviewer questioned the potential to lose some of the critical detail in moving to the spatially stratified approach employed by ALCES®. Recommendations are made for model validation, sensitivity analysis and uncertainty analysis.

The review of the natural disturbance module was highly critical of both the approach and the parameterization. However, the reviewer clearly states that the documentation provided is not adequate to provide a comprehensive review. There is a lack of transparency in the assumptions, terminology and parameterization of this module. It is not possible to make any final statement about this sub-model without further refinement of the documentation followed by another review.

The review of non-native species invasion was a relatively positive assessment of the sub-model with affirmative responses to the five guiding questions. The primary concerns relate to the paucity of 'hard' data on the rates, mechanisms and other characteristic of invasive species introduction and expansion. The expert-based approach was recognized as particularly useful in this case. The sub-model documentation was found to be clear on the sources of professional/experiential opinion. Data were stronger for invasive species occurrence (e.g. presence/absence) than they were for dynamic processes. The reviewer advocates an adaptive management approach whereby the model is continually refined as new information becomes available. In addition, the reviewer provides suggestions for the development of a more site and species specific model developed through the use of probability factors. The reviewer concludes that the sub-model for use at a strategic planning level, especially for issue identification. However, the lack of quantitative data makes the predictive capacity of the sub-model less certain.

A list of specific questions and issues to be addressed is included in the summary section of this report. Many of the issues identified for all sub-models pertain to the need for more explicit documentation of assumptions and rationale/justification for selecting model parameters. The quality of the documentation may have impeded the ability of reviewers to fully address the guiding questions for the review. Subsequent materials for review should be more comprehensive and transparent with respect to sub-model development and the overall function of ALCES®.

Contents

Introduction

Alberta Environment, in collaboration with other relevant ministries, is developing a regional sustainable development strategy for southern Alberta. The purpose of the program is to develop "a vision of the future of Southern Alberta and the desired environmental, social and economic benefits for the region, and then address the issues and follow a plan to achieve the vision" (Alberta Environment 2003).

The Southern Alberta Sustainability Strategy (SASS) managers have chosen to employ a computer simulation model known as ALCES® (A Landscape Cumulative Effects Simulator, see: Forem Technologies http://www.foremtech.com/products/pr_alces.htm) as one of the tools in developing the strategy. ALCES® is a stock and flow (systems dynamics) model constructed in a STELLA® modeling environment. The model operates by establishing relationships (pathways and rates of flow) between entities (stocks) of interest (e.g., land-use and land-cover types) and then simulates the changes in the entities over time. ALCES® enables resource managers, industry, society and the scientific community to explore and quantify the cumulative, dynamic effects of human land-use practices and existing natural disturbance regimes. ALCES® contributes to a regional strategy through its use as an exploratory tool to identify emerging regional issues and opportunities, and by examining the potential implications of trends and policy choices under a range of future scenarios. This model is driven through a collaborative visioning process that ultimately contributes to planning for regional sustainability.

ALCES® requires customization for application to different landscapes. The modular architecture of the model allows for individual sub-models (modules) to be added, deleted or developed and parameterized for specific landscapes. ALCES® has been customized for SASS to operate with 12 sub-models. The following is list identifies the sub-models and the key variables that have been identified as important indicators of landscape change:

1. Landscape Composition
   - % of Landscape in Native Habitat
   - Anthropogenic edge
2. Meterology, Aquatics, & Industrial Use of Water
   - Water Quality Index
   - Change in Water Demand
   - % of Flow used for Land use
3. Agriculture & Livestock
   - % of Landscape in Productive Cropland
   - % of crop production from Intensive Practices
   - % of Livestock from Feedlots
   - Number and area of Exotic invasive species
   - Livestock and Crop Production
4. Plant Community and Rangeland Community Structure Dynamics
   - Number and area of Exotic invasive species
5. Energy & Mining
   - Hydrocarbon production
6. Forestry
   - % of Land base in each of forest trajectories
   - Age class structure
   - Number and area of Exotic invasive species

- Softwood and Hardwood Production

7. Wildlife Habitat & Community Richness
   - % of Landscape in Native Habitat
   - Habitat effectiveness for wildlife indicator species
   - Sustainable Wildlife Harvest Rates
   - Native and Anthropogenic Vertebrate guilds
8. Transportation
9. Human Populations and Settlement
   - Size and Distribution of human population
   - Tourism Activity Days
10. Fire & Insect Disturbance Regimes
11. General Industry & Electrical
12. Tourism/Recreation & Hunting/Fishing.

The Miistakis Institute was contracted by Alberta Environment to coordinate an independent, expert, technical peer review of the methodology behind four ALCES sub-models, and an assessment of whether or not they allow for useful and reasonable simulations of natural resources over 50 year time-periods in support of current land and resource management decisions (see appendix A for `Terms of Reference`). The review of 4 modules, which were prepared specifically for the SASS, included:

- Wildlife and Biodiversity,
- Invasion of Non-native Plant Species,
- Natural Disturbance (fire and insects), and
- Rangeland Plant Community Structure Dynamics.

The initial terms of reference also required a review of the module on water quantity, but it was subsequently removed from the list by Alberta Environment.

Alberta Environment provided an initial list of potential reviewers which was supplemented and finalized by Miistakis with approval from Alberta Environment. The criteria for identifying reviewers included the following characteristics:

1. The reviewer should have no professional stake in the outcome of the review. That is the reviewer must have an 'arms length' relationship with Alberta Environment.
2. In general University based experts should be chosen because they are least vulnerable to negative reactions that might arise from unfavourable reviews.
3. The reviewer must be an expert in the subject area.
4. Attempt to recruit Alberta based academics as reviewers since they would be most familiar with the study area and the critical issues surrounding sustainable development in the area.
5. The reviewers must not have had a close working relationship with any of the authors. This included acting as an academic supervisor for a graduate degree.

In addition, potential reviewers had to be available to complete the review within the proposed timeframe. In the case of Rangeland Plant Community Structure Dynamics, two reviewers were selected. All other model components were reviewed by one individual. The following is the list of individuals who provided a review (see Appendix C for biographies):

- Edward Bork, University of Alberta (Rangeland Plant Community Structure Dynamics),
- Mark Hebblewhite, University of Alberta, (Wildlife and Biodiversity),
- Barry Irving, University of Alberta (Rangeland Plant Community Structure Dynamics),
- Edward A. Johnson, University of Calgary (Natural Disturbance),
- Walter Willms, Lethbridge Research Centre, Agriculture and Agri-Food Canada (Invasion of Non-native Plant Species).

Potential reviewers were contacted by e-mail and/or telephone to solicit their participation in the project. The initial contact included a letter explaining the details of the review (Appendix A) and a copy of the module documentation to be reviewed (available from Alberta Environment). Reviewers were offered remuneration commensurate with the level of time and effort required to complete the review. Additional documents and clarification were provided by the contractor as necessary. Reviewers also received an electronic copy of the document *Modeling the Impacts of Landscape Change in Southern Alberta using ALCES (A Landscape Cumulative Effects Simulator)*, an overview prepared by Alberta Environment and were directed to the Forem Technologies website for further information on ALCES®.

Questions to be addressed by the reviewers included:

- Is the approach/methods used sound?
- Are assumptions valid and/or justifiable in light of the information available?
- Are data used in an appropriate way?
- Does the module address critical aspects of the module subject area?
- Does the module address critical issues for strategic planning in southern Alberta?

The reviews provided by each of the experts listed above are provided *verbatim* in the following sections. The style, length and nature of the reviews differ significantly due in part to the diversity in the amount and type of documentation provided. For example, the documentation for the *Disturbance Module* consisted of a 5-page text document whereas the *Wildlife and Biodiversity Module* included a 78-page report and an accompanying 21-page summary of the grizzly bear component.

A summary of comments and questions arising from the reviews is provided by the compiler in the final section of this document.

Review of: Modeling Wildlife and Biodiversity for the Southern Alberta Sustainable Strategy (SASS)

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Introduction

I independently reviewed Scheick, J. 2004. *Modeling Wildlife and Biodiversity for the Southern Alberta Sustainable Strategy (SASS)*. Between October 1st and Nov 2nd, 2004, I reviewed this and related reports for Dr. Mike Quinn, Misitakis Institute, EVDS, University of Calgary, as part of an AB-SRD sponsored review of the Southern Alberta Sustainability Strategy (SASS). The list of reports I reviewed as part of this includes:

- Scheick, J. 2004. Modeling Wildlife and Biodiversity for the Southern Alberta Sustainable Strategy (SASS)
- Nielsen, S., and Boyce, M.S. 2002. Grizzly bear habitat selection and mortality coefficients of Southern Alberta: estimates for the Southern Alberta Regional Strategy (SARS) – ALCES Project
- The AB-SRD website materials on the Southern Alberta Sustainability Strategy at http://www3.gov.ab.ca/env/regions/southern/strategy.html
- The ALCES website http://www.foremtech.com/
- Relevant literature including key words habitat suitability index, cumulative effects assessment, and Delphi-approach combined with conservation and ecology key words.

Scope of Review

I was asked to consider these questions in my review:

- Is the approach/methods used sound?
- Are assumptions valid and/or justifiable in light of the information available?
- Are data used in an appropriate way?
- Do the module address critical aspects of the module subject area?
- Does the module address critical issues for strategic planning in southern Alberta?

I address these questions below in order, with specific answers, make specific comments on each section, and then make conclusions.

Overview

This wildlife modeling report is a component of the regional resource overview for the SASS regional planning area. Reviewing the SASS literature available to me on the Internet and in this report, I had a difficult time determining specific goals and objectives of the SASS. The closest definition I found was “to help understand future regional issues and opportunities concerning the sustainable use of natural resources and the environment”. The definition of sustainability has key importance for evaluating the results of this wildlife module or any other module, and these goals, objectives, and definitions should be included in all future SASS literature. The goals of the report are broad and ambitious, to model future wildlife and biodiversity responses in the entire Southern Alberta study area. The report 1) identifies focal species and focal species groups using the Delphi approach, 2) decided how to model species using ALCES, 3) used a Delphi approach to estimate habitat suitability (H.S.I.) models which would be implemented in ALCES, and 4) implemented these H.S.I. models for each species and/or species group under 2 future scenarios. Data-driven models were used for 1 species, the grizzly bear. In addition, the report includes a ‘coarse-scale’ modeling exercise based just on projecting different landcover types into the future.

Strengths

The author notes the main strengths of the Delphi-based approach combined with the ALCES model: 1) cost effectiveness compared to species specific natural history studies, 2) speed of collation of HSI models compared to intensive data analyses on a by-species level, and I see several others; 3) ease of implementation in the ALCES framework vs. reformulating data from other approaches (i.e., the Grizzly bear module), 4) participatory buy-in of collaborating regional and provincial biologists and managers, 5) simple, intuitive results that have few interactions or interdependencies and that are easy to understand, and 6) evaluations over the entire SASS study area are made easily possible.

A. General Methodological Areas of Concern

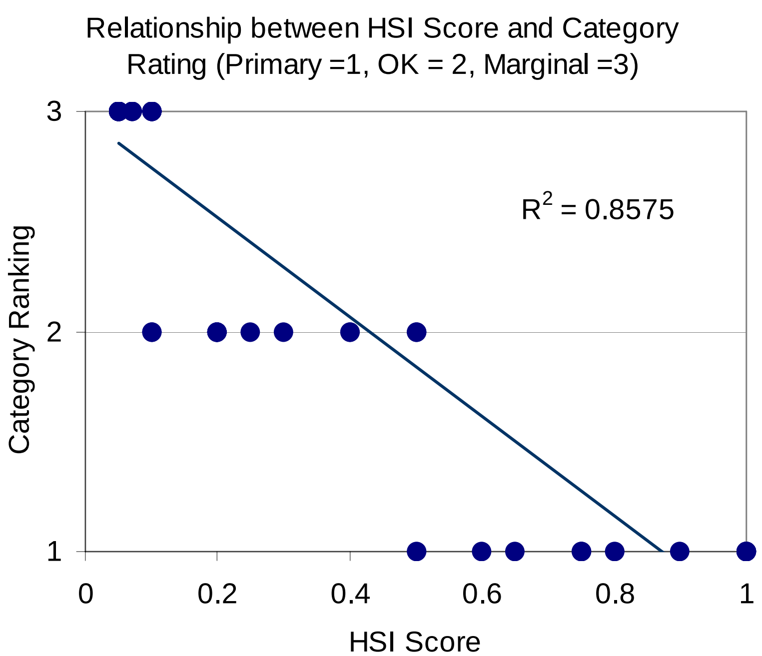
1) Use of the expert-opinion, or Delphi approach, without including uncertainty.

The Delphi approach has a long-standing role in focal species selection (Beazley and Cardinal Jun; Hess and King 2002), and its use is well supported in such applications. I doubt much improvement would result from revising the focal species selection procedure. However, the utility of the Delphi for H.S.I. modeling has been contested mainly because of difficulties in validating models (Loiselle et al. 2003; Rothley 2001; Brooks 1997) and the failure to incorporate variation in HSI values (Johnson and Gillingham 2004; Roloff and Kernohan 1999; Bender et al. 1996; Dettki et al. 2003). While this report acknowledges differences in opinion in Delphi scores, there is no attempt to incorporate uncertainty into the modeling framework (Johnson and Gillingham 2004). As such, this effort falls short of modern, accepted, published and government guidelines for the development and implementation of HSI models, for example, by the USFWS (Roloff and Kernohan 1999; Holthausen et al. 1994). Similar large-scale ecosystem level cumulative affects assessments are making use of Bayesian Belief Network approaches where model outcomes are associated with uncertainty estimates (Lehmkuhl et al. 2001). Incorporating uncertainty analysis into HSI modeling has been an accepted standard for more than a decade, and ignoring it constitutes one of the main failings of the modeling effort. I conclude the review with a section on uncertainty analysis.

2) Inconsistency between species in H.S.I. scoring.

For the species group models, efforts were made to ensure that the ratings were consistent between species; i.e., a poor rating was always 0.1-.2, for example. However this was not done for the individual species models. As a result, there were inconsistencies between species of what Primary, OK, and marginal ratings were. This resulted in inconsistent discrimination between these categories between the 3 different species, as illustrated in Figure 1. In Fig.1 I plotted the 3 categories, primary =1, OK = 2, marginal =3 vs. the H.S.I. ratings, showing the overlap between categories. Scores of 0.5 were shared between category 2 and 1, and the most overlap occurred for all of category 3 with 2. I recommend adopting consistent categorical definitions for H.S.I. habitat rating scores for each individual species model and the group species models. In its current state, it is not possible to quantitatively compare species responses because of these problems.

Figure 1.



3) Index of Change Relative to Historic Baselines

I found the most serious problems in interpretation and methodologies in the calculation of the INDEX OF AVERAGE CHANGE, the main management metrics of the report.

The historic baseline approach has 2 main assumptions; 1) Removal of all modern human development and impacts in the ALCES model accurately reflects historic conditions, and 2) ecological relationships between model components at present was the same 100+ years ago. These implicit assumptions were not described in the report, but are logically assumed in the modeling. Both assumptions are critical to the calculation of the INDEX OF AVERAGE CHANGE, which form the main modeling results generated from this exercise.

Both assumptions are certainly not true. The failure to even mention the absolutely pivotal, keystone role of both First Nations and the regions dominating ecological process prior to European colonization, the seasonal migration of hundreds of thousands to millions of Bison (Knapp et al. 1999; Geist 1971; Hewitt 1921) belies that the consequences of these assumptions were ignored in the report. First Nations had dramatic impacts on the abundance and distribution of all the species considered through burning and hunting (reviewed in Krech 1999). Thus, in the ALCES modeling framework, setting all modern human uses to zero does not constitute historic conditions. If variation in fire incorporates this native burning frequency, it was not clearly outlined in this report, but fire is only one non-interacting component of this role of natives in the historical baseline constructed in the model.

Bison were the dominant ecological force in the prairie ecosystem prior to settlement (1999; Knapp et al. 1999; White et al. 2001; Truett 1996). Assuming absence of human use in historic conditions would reset all the grazing levels in the Alces models to 0, from what I can tell in the report, obviously a erroneous assumption that ignores the perhaps millions of bison present at that time. Removal of Bison has dramatically impacted almost all prairie ecosystem processes, from flooding, grazing, fire, succession, community dynamics, and nutrient cycling (Knapp et al. 1999; Geist 1971; White et al. 2001). In fact, one of the ecoregions considered for conservation in the SASS model, the aspen parkland, may not have even existed prior to European settlement because of the dominating effect of bison herbivory, combined with Native fire (Campbell et al. 1994). Thus, assuming ecological relationships between ALCES model components stays constant over time spanning across 100's of years and through massive ecological changes is a major flaw of the report. While some components include an historic range of variation concept through the effects of variation in fire and vegetation ALCES sub-components, these are still linear interpolations from present conditions to historic conditions, ignoring the key role of bison grazing, native burning, and ecological interactions which no longer exist. Perhaps someone could argue that this is semantics, however, I show below that this interpretation is perhaps key to the calculation of the INDEX OF AVERAGE CHANGE.

Therefore, use of the % change from Pre-European times in the calculation of the INDEX OF AVERAGE CHANGE is fundamentally flawed. Managers cannot hope to manage for pre-European times, nor are the results from these pre-European times convincing in the least for the reasons I outline above. This index is misleading and inflates the projections for the future changes; next I outline why I think this is the case.

The report concedes "the relative importance of these two components (changes from historic and changes in the future) is difficult to determine, but for simplicity they have been treated as equally important in the analyses…". This is not a convincing argument why both should be included, why they received equal weighting (by dividing by 2), or how the averaged index relates to management in the future. Conceivably, it is the long-term rate of decline in H.S.I. indices that is important, so I might argue instead for geometrically weighted mean rates of decline, or overall % decline. However, because of the problems outlined above with the assumptions of the historic baseline approach that are nearly insurmountable, I recommend just focusing on just the future % changes. To illustrate how dramatically doing so will change the implications of the report, I have approximated this from figures in the text in Table 1 for the species-specific models.

Table 1. Comparison of projected % change from historic to present, future change under optimum (no development) and status quo conditions, the Index of average change calculated from the report, and just the range of future % changes under the optimum and status quo options.

Species	Pre-European % Change	Future % Change – Optimum	% Future Change – Status Quo	INDEX OF AVERAGE CHANGE	Index of Future change
Ferruginous Hawk	-60%-80%	-5%	0%	-48%	-0 to -5%
Prairie Rattlesnake	40-57%	-7%	-23%	-61%	-7 to -23%
Sharp-tailed Grouse	75-80%	-5%	-10%	-66%	-5 to -10%
Grassland species	-44%	-14%	NA	-34%	-14%

Table 1 illustrates the distortion caused by including the pre-European % change with the future changes. For some species, such as Ferruginous hawks, future scenarios projected almost NO decline in the H.S.I. value (see Fig. 4.4 in the report)! Yet the averaged decline, based on the faulty assumptions outlined above indicates a –48% decline. Managers who cannot hope to manage for historic conditions will not be moved to action by these small percentages. Similarly, for many of the coarse-filter approaches, outlined in Fig 10.1, and 10.2, show NO decline from present to future conditions. Why then, would a manager be convinced by these analyses to undertake dramatic action to reverse human development? To achieve some lofty historic baseline? The small magnitude of many of the declines from present to the future under either the optimum or status quo reveals problems, I believe, with the modeling process. Either the future scenarios for development, which were not described in the report, were so moderate that almost no development is projected to occur in the SASS area, or there are perhaps fundamental problems with either the H.S.I. models, or the model structure. It is impossible to say without more detailed model and subcomponent description, nor without UA or SA (see below). This is concern over the relatively small predicted future changes is especially true because I think it fairly common knowledge among biologists working in the SASS region that prairie ecosystems are in sharp decline. Reconciling these insights from biologists with the small projected % changes in future conditions is a major criticism of the report.

4) Biodiversity

Although biodiversity appears in the title, headers, introduction, and abstract, substantive Biodiversity content is notably absent. Biodiversity is defined as "**Pertaining to the diversity and frequency of organisms in a given area** (my emphasis)." (Rosenzweig 2003). Accordingly, I was expecting to see a section summarizing species diversity per unit area using alpha, beta-diversity, species richness, evenness, etc. That there was no specific section dealing with biodiversity in the report, and instead merely a collection of individual species models, and some community level responses reveals that effects of future development on biodiversity were ignored. Therefore, I recommend the word biodiversity be removed from the document, and perhaps replaced with terrestrial/mammalian communities, etc. A sceptic would say Biodiversity was added after the fact. This really was just a terrestrial wildlife and wildlife community report.

The lack of substantive section on biodiversity, in fact, further reveals the limiting non-spatial nature of ALCES (see next section). The definition of biodiversity, in a per unit area measure, poses significant problems for a non-spatial modeling framework. Measures of species diversity have been intimately connected with consideration of space from its inception (Rosenzweig 2003); the 2 concepts are almost inseparable. Therefore, biodiversity/unit area is the critical measure. In this report, biodiversity of the SASS landscape, as such, remained constant over time regardless of the scenarios in so much that none of the modelled species went extinct. Smaller spatial scale processes of local extinction, and thereby changes in local biodiversity are not captured using ALCES. Unless ALCES can track patch–level species diversity, notwithstanding species-specific scale-issues, a non-spatial framework will not be useful for addressing Biodiversity in the SASS area.

5) Non-spatial modeling framework

The ALCES model framework is fundamentally a non-spatial model, and several places in the report the author recognizes this limitation, but does not suggest what the costs of this are to the results. For example, in the coarse filter modeling, it is recognized that "coarse filter modeling in the SASS study area should be stratified by amounts of human disturbance", …but " due to constraints within ALCES (non-spatial), stratification … was not done". Reducing spatial data from the grizzly bear model, for example, probably led to reduced predictive power due to the loss of highly important spatial covariates for this species (patch characteristics, dispersion, etc, (Nielsen et al. 2003), and this criticism applies broadly to other species. For example, habitats >25km from major river valleys were considered 'extra-limital' for rattlesnakes, yet this rule assumes a 1/0 step function of consistent abundance <25 km from rivers, and abundance =0 >25km from rivers. This would overestimate the amount of rattlesnake habitat, when in reality the probability of occurrence is a continuous declining function of distance from river.

By ignoring space, differences in scale between species are difficult to incorporate or understand. For example, obvious scale differences between grizzly bears and sharp-tailed grouse mean that each species likely responds to the grain and scale of the environment differently. Grizzly bears assess habitat quality orders of magnitude greater than grouse, and thus, would require large blocks of contiguous habitat than grouse, for example. In ALCES, it is not clear if it is possible to address the critical scale dependency of the habitat modeling across different species. For this reason, I am not sure if cross-species comparisons in % habitat effectiveness changes are valid. Non-spatial models, moreover, cannot say anything about connectivity, spatial dispersion, etc., so I was confused to see some of these types of parameters listed in tables in the report. Furthermore, ALCES cannot provide guidance for identifying 'benchmark' or new protected areas that might be required to balance wildlife losses in the long-term.

The attempts to address landscape and patch level dynamics in the model are difficult to understand, given the material presented. How were patches tracked in a non-spatial model? Using mean patch size across all patches in the landscape? Median? Distribution of patch sizes in landscapes is typically non-normal, instead following a log-normal, Poisson, or negative binomial distribution dependent on the level of fragmentation (Turner 2001). Means are almost useless for characterizing these distributions of patch sizes. Regardless, only 2 covariates were measured at the patch scale; vegetation structure, and water sediment load (pg 14), and the author recognizes the limited scope of the patch scale framework.
One of the most spectacular examples of the failure of non-spatial models in H.S.I. modeling is on elk. For decades managers have used road density to discount elk habitat effectiveness across the NW and western Canada (Jones et al. 2002; Buckmaster et al. 1995; Lyon 1983; Schuster et al. 1985; Rowland et al. 2000) using a simple linear decline as a function of road density that is similar, for example, to Figure 5.1 in the report. The basis for the road density relationship was estimated from field data of elk distribution (pellets, telemetry) but regressed vs. distance from road. The elk use- distance from road required spatial data, but was aggregated to the non-spatial surrogate of road density, then applied as a management tool across much of the Pacific Northwest. However, in the only real model validation for this H.S.I. model conducted under rigorous experimental conditions in Starkey Forest Experimental Research Station (Rowland et al. 2000), there was no relationship whatsoever between elk use and road density. Rowland et al (2001) caution that this was because of the spatial context of roads. Elk use of areas close to road depended critically on the spatial arrangement of roads. Therefore, decades of H.S.I. models that used this simple, non-spatial management tool, may have inadvertently had biased or inaccurate results. In fact, this relationship may explain the poor performance of many elk H.S.I. models (Jones et al. 2002).

It seems to me that perhaps 5-10 years ago, convincing arguments could be made for adopting a non-spatial approach merely in terms of raw computer processing power. However, larger and larger landscapes up to 100,000's km2 are being successfully modeled in spatial cumulative effects assessments (Carroll et al. 2001; Nielsen et al. 2002; Lehmkuhl et al. 2001; Kerley et al. 2002) in GIS platforms. GIS platforms offer advances in that they are spatially explicit, can adapt to multiple spatial scales (Store and Jokimaki 2003), can be combined across species (Ruth et al. 2003; Burley 1989) and are readily adapted to testing, validation, uncertainty and sensitivity analyses (Johnson and Gillingham 2004a,b; Crosetto et al. 2000). Moreover, spatial tools to model the SASS study area are becoming available; for example, the Alberta Ground Cover Classification Landsat classifications produced by Sanchez and Hall at the University of Alberta include the SASS area.

Finally, no description of where the habitat categories came from in the ALCES model. My understanding of them is they come from AVI data, which have noted problems and lack of detail for non-timber resources, such as coulee environments, badlands, shrublands, etc. While AVI is used obviously by the AB govt, some discussion of these issues is warranted, and indeed, would lend itself well to uncertainty analysis.

6) Simplistic Human use modeling scenarios

The most useful part of a modeling process is comparing the relative effects of different strategies/parameter values, etc., on projections. This report makes only 1 comparison between 'status quo' and some undefined likely future scenario. No details about this future scenario are presented. This makes it tough to evaluate the utility of the model, I recommend the full human use model components are presented. Moreover, the simplistic human use models as incorporated into this report will offer almost absolutely no guidance or direction to management. For example, if grassland species are declining at –50% by 2050, what should managers focus on? Agriculture? Oil and gas? Recreation? Water diversion? By lumping 'human-use' into one category, management will be unable to tease apart the relative influences of various land uses, which will surely vary spatially/across habitat types within the SASS area. Because the real worth in this, and any modeling process, is the relative comparisons between scenarios, I recommend developing many more future scenarios for evaluation. These scenarios could examine, for example, status quo, 100 reduction ('optimal') in this report, 50% reduction, 150% current, etc. Moreover, the model could then vary the landcover extents of certain human use activities to evaluate potential management actions. For example, road building within river valleys could be reduced, and this effect could be evaluated specifically for Rattlesnakes. The true test of the ALCES models' sensitivity to these sorts of future simulations will reveal its utility to management. On a presentation on SASS linked to the AB-SRD web page, I read that this approach will allow managers to adapt management to different human uses in different habitats, and will allow evaluation of the mechanisms of species decline. Unfortunately, this is not true with the present, overly simplistic, vague, and undirected human use scenario. I strongly recommend considering multiple human use scenarios, and that this becomes the main focus of the project. Doing so will ensure a useful product for management.

Assumptions

I outlined the 2 major assumptions made in the calculation of the Index of change above. In general, there was not enough information present in the report about assumptions inherent in the modeling process. Where assumptions were given, I found some of them questionable, and of minor importance. Inherent assumptions of the modeling process were often not explicitly stated. I do so below:

1) ALCES model structure – I found the report lacking in the description of the salient features of the ALCES model framework. For example, there was no information about the habitat types used, integration rules over space-time, and aggregation rules for summing habitat types over large areas, or the time units modeling was conducted. As such I found it difficult to actually address the 3rd question asked of me, namely were data used in an appropriate way.

2) Exotic Weed ALCES Subcomponent- there was no information presented in this report to evaluate the exotic species subcomponent model.

3) Human Growth Scenarios – absolutely critical to every single modeling effort in this report is the anticipated future growth in human resource extraction, populations, recreation, yet no where in this report is information about these scenarios presented. This is a crucial limitation of the utility and repeatability of this report. This may also be related to why % change in future scenarios is so slight.

4) Functional Responses – This and other HIS efforts make the implicit assumption that preference, indeed habitat suitability itself, remains constant as availabilities change. This is simply not always true for many species (Mysterud and Ims 1998; Osko et al. 2004). This change in preference as availability changes is known as the functional response in

habitat selection. While a difficult assumption to get over in H.S.I. modeling, this assumption is critical and should be explicitly stated.

5) Section 2 - Pg 13- I doubt the assumption of no effects of anthropogenic edge in prairie habitats is true. Further, in the same section the report assumes only non-native/exotic plants occur in the prairie region, not in the Forested areas. This is not true, and increase in invasive species in these areas, for example, spotted knapweed, poses a significant future threat to wildlife in this region.

B. Section Specific Comments

Section 2.0 – Species Selection

- The report does not list endemism (i.e., the % of the species range that occurs in AB) as a factor in the list of criteria used to identify focal species. However, this factor was used repeatedly to justify or exclude certain species. For consistency, add endemism to the factors.

- The justification to exclude Sage grouse from the modeling efforts, and include Sharp-tailed grouse, because of concerns that Sage grouse will continue to go extinct regardless of management intervention, seems counterintuitive and circular. By relegating Sage grouse to extinction, and therefore any other species that may be similarly endangered, this report is consistently biasing their results more positive than they would be if they included Sage grouse. The geographic range argument doesn't seem to be decisive for Sage grouse because Grizzly bears are equally restricted in their range. Discussions with M.Quinn indicated there is an ongoing, somewhat parallel process for Sage grouse being conducted under the auspices of the species recovery action group, and so in some ways, perhaps my comments are not necessary. However, if similar Sage grouse modeling is occurring, it should be more thoroughly reviewed and discussed in the SASS wildlife report. That it isn't suggests continued lack of regional integration in Alberta sage grouse recovery efforts (Aldridge et al. 2004). Clearer links and explanations for how the SASS ties to ongoing SARA and other management efforts are required. Moreover, including Sage grouse, for which much is known of their population rate of change, would help validate ALCES models.

- As a general editorial comment, I found the report jumped from which species were selected straight to the coarse-filtering approach. There was no discussion of the methods used for the species-specific modeling. Because much of the species specific section in repetitive (and in fact, was cut and pasted, see Sharp-tailed grouse section), it would improve the readability of the report to proceed following the normal IMRAD scientific communication format.

Section 3.0

- I completely agree that a sufficient, statistically rigorous monitoring program is a crucial component of first validating, and then implementing the results of any ALCES modeling conducted in the SASS area. Furthermore, I strongly support the adoption of a true adaptive management framework as defined and reviewed for SE Alberta in Aldridge et al. (2004) as part of the SASS framework.

Section 4.0

- Pp 17- What are the units for grazing intensity? Grazing % off take during a certain time period? Animal unit months (AUM's)? The effects of grazing on biodiversity vary critically on the timing, duration, and spatial pattern of grazing by large herbivores (Senft et al. 1987), and thus, collapsing all these myriad processes to % grazing intensity and then predicting linear or unimodal fixed relationships for single or group species is difficult.

Section 5.0 Rattlesnakes

- The assumption that recent observations equal the upstream limit of rattlesnakes assumes that species range has not retracted during recent range collapse, which seems a false assumption. If this is true, results may be biased positively.

- See my earlier comment regarding the 1/0 step function abundance rule used for defining Rattlesnake range.

- P23 – is 'rough country' a technical term or habitat type identified in the SASS landcover map?

Section 6.0 Sharp-tailed grouse

- Pg 29 – It is unclear here, and in the other species accounts, what statements are made that stem from the expert opinion, and what statements might be supported by the scientific literature. For example, the statement "benefits of grazing peak at about ½ maximum recommended stocking density, because at that point availability declines…". This seems like an overly exact quantitative statement that could not be derived from Delphi approach.

- To my knowledge, ST grouse are a game species, and therefore, one obvious scenario that could be modelled which would reduce the discounting rate for increased human access, is a reduction in hunting licences sold/managed for Sharp-tailed grouse. This should be included.

- It is clear that the species-specific accounts were cut-and-pasted. For example, in the Sharp-tailed grouse account, they are referred to as Rattlesnake. I found much of the species accounts repetitive, and made editorial suggestions above to this effect.

Section 7.0 Grizzly Bear

- Reviewing the data-based models for grizzly bears I can find no major problems with the logic, methods, statistical approaches, or inferences drawn from the report. I agree with many of the caveats given by the authors, and moreover, think that the method of reweighing key southern Alberta habitat types based on preliminary data from Carita Bergman is valid.

- I observe that the magnitude of the change in historic to present conditions for grizzly bear habitat use did not change nearly as much as HSI values for other species based on Delphi-based approaches. This makes me question whether differences between approaches pose problems.

- As part of the overall validation scheme, I think it unfortunate that for the species for which you could construct BOTH a data-driven and Delphi approach model, the report did not (see also discussion of sage grouse above). Doing both methods would have allowed quantitative assessment of the Delphi-approach, and if the results were positive (Loukmas and Halbrook 2001; Mitchell et al. 2002), would have instilled greater confidence in the overall Delphi-based models.

- After reading the special report and discussing it and other efforts in the Northern East Slopes planning region with Scott Nielsen, University of Alberta, I have the following comment that relates to the general problem of ALCES being non-spatial. I think the lack of predictive power of the habitat use model for grizzly bears reflects more on the quality of non-spatial data being used in ALCES vs. the spatial resolution of GIS-based RSF modeling approaches. For example, to the best of my knowledge (because no information was provided for me to be sure) ALCES is based on the provincial AVI maps, notoriously poor for classifying non-timber plant communities, and cannot incorporate spatial covariates or their interactions with habitat types, which often drive habitat prediction models for species like grizzly bears (Nielsen et al. 2002).

Section 9.0 Grassland specialists

- I think the overall grassland species approach is generally valid, although I do have some general comments.

- First, it was not clear to me how the individual slopes were estimate for the relationships for each species – was each species done separately? If so, how does this differ from the individual species models?

- Second, it is not clear to me how the individual species HSI-models were combined – additively, cross products? Evidence from the literature suggests the cross products of individual species HSI models (Vadas and Orth 2001) produce the best community habitat effectiveness models.

Section 10.0 Coarse-filter approach

- The forestry component suffers from a fatal flaw of not explicitly addressing forest fires in terms of regenerating old growth forests (witness Lost Creek Fire), unless the details were not presented in sufficient detail. Ignoring fire in harvest modeling has proved problematic elsewhere in AB (Timoney and Lee 2001).

C. Including Model Validation, Sensitivity Analysis, and Uncertainty Analysis: Suggestions for future efforts

I define model validation to be an out-of sample data test of the predictive accuracy and bias of a predictive model (Boyce et al. 2002; Hastie et al. 2001). I define sensitivity analysis (SA) to be the systematic evaluation of the most important parameter in a predictive model algorithm in determining the final model results (Johnson and Gillingham 2004a; Crosetto et al. 2000). I define Uncertainty analysis (UA) to be the evaluation of the effects of variation in model parameters (uncertainty), either coming from process or sampling variation, on the magnitude of model results (Crosetto et al. 2000; Wisdom and Mills 1997).

Model Validation

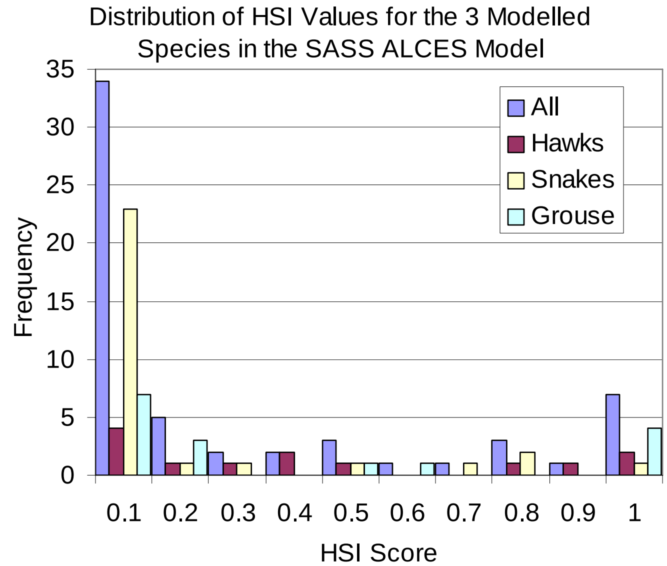
1) As the author notes in the abstract, model validation should be the 'next' step in the wildlife process. I would argue that data exist already, in some capacity, for conducting species-specific validation within key study areas of SASS, and this should be the crucial next step. For example, ongoing graduate and agency research is presently being conducted on Sharp-tailed Grouse (Manzer, UofA), Prairie Rattlesnakes (Jorgenson – UofC), Grizzly Bears (AB-SRD, Stenhouse), pronghorns (ACA), and Ferruginous Hawks (sighting database in SE AB, Aldridge, UofA). Moreover, historical databases, incidental sightings, and other agency data should be used to full advantage for HSI model evaluation. I see no point in repeating the Delphi-based approach for validating wildlife models per se unless the main purpose of repeating the Delphi-based approach is for evaluating uncertainty in HSI scores (Johnson and Gillingham 2004a; Roloff et al. 2001) (See uncertainty/ sensitivity analysis below).

2) Non-spatial Cumulative Effects - ALCES poses problems for validating individual species models because of its non-spatial nature. An immediate problem of converting species-specific studies in portions of the SASS study area is how to aggregate spatial data to non-spatial data as modelled by ALCES. Scale-specific problems will differ between species. More importantly however, as discussed in the grizzly bear section 7.0, aggregating spatial data to non-spatial data results in considerable loss in predictive power, which will magnify when combined with scale and other problems in the non-spatial ALCES. For example, how are spatial parameters estimates in ALCES for parameter distributions that are non-normal? Without original spatial distribution data, for non-normal data, aggregating by mean values may bias parameter estimates.

Uncertainty Analysis

Many authors (Roloff and Kernohan 1999; Bender et al. 1996; Johnson and Gillingham 2004a) outline methods to evaluate the effects of uncertainty in H.S.I. models. Uncertainty can enter H.S.I. models starting with individual H.S.I. habitat ratings, specification of the relationship between a variable and the H.S.I., variation in how different variables are combined in the modeling framework, variation in the spatial or non-spatial implementation of the final model, etc. Some authors consider variation in the original Delphi scores for habitats to be one of the most important sources of variation (Johnson and Gillingham 2004a). For example, Johnson and Gillingham (2004) found assumed variation in expert opinion resulted in large declines in the area of high (up to -85%), and moderately high quality habitat (-68%). The distribution of variation in H.S.I. scores had the largest effect on the amount of high quality habitat.

In future efforts, I recommend SASS consider and incorporate variation in Delphi scores,



either from non-parametric bootstrapping of actual Delphi scores (non-parametric because of small number of 'experts'), or from a distribution of Delphi scores such as uniform. Figure 2 shows the distribution of H.S.I. scores across 3 species, and suggests a uniform distribution may be appropriate. Including this variation throughout the rest of the modeling framework would allow an evaluation of how important the variation is, and would alleviate concerns raised about the problems of the Delphi approach.

Fig. 2

Sensitivity Analyses

I recommend sensitivity analyses be conducted to examine to which model component (H.S.I. values, expected relationships, model structure, vegetation component, human development, etc.) the results of the simulations are most sensitive too. This would help identify key processes to focus management and additional research and modeling efforts on. For example, it would be relieving to know that for some species, the exact nature of the relationships between H.S.I. values and some covariate are unimportant. Methods to include SA in H.S.I. models are described by (Roloff 2001; Crosseto et al. 2001; Johnson and Gillingham 2004).

D. Conclusions and Recommendations

I have tried to present a fair, rigorous, and comprehensive review of the Wildlife and Biodiversity module for the SASS. I outlined above many problems with the modeling approach that require additional work before the results can be incorporated into management. I recommend addressing the following critical components:

1) Biodiversity- this report does not address biodiversity in the SASS area – recommend removing all references to Biodiversity from the document, or adding new 'patch' level biodiversity measures into ALCES modeling framework.

2) Calculation of Index of Change – for the reasons outlined above, I recommend removing the calculation of change from historic to present from the document, instead focusing only on the future changes. However, much of these future changes are small and trivial for some species, which reveals potential problems with the model structure that I outline above. This will need additional work.

3) Adopt consistent H.S.I. rating categories for species-specific models – as illustrated in Fig. 1 of this review.

4) Include uncertainty analysis by simulating variation in Delphi scores, the easiest approach to uncertainty analysis.

5) Include description of key components in this module – I found it difficult to fully evaluate how ALCES was working, and therefore fully evaluate the approach. Moreover the weed submodel, the human use submodel, and other key components were absent from the report.

6) Detail the Human use submodel – the most useful purpose of any model is comparing relative differences between different strategies. Thus, I recommend detailing the human use submodel fully in the report. Furthermore, considering (really) only 1 model of increasing status quo development limited comparative insights from the modeling process. I recommend considering at least 3 different human use strategies. I also recommend evaluating differential changes in subcomponents of the human use model, for example, just changing one of the agriculture, oil/gas, etc.). This would allow relative comparison of the relative impacts of the different industrial activities on the SASS landscape.

Once these key areas are revised, I recommend

7) Validate H.S.I. model results – Identify ongoing research, observational datasets, and other sources of data to validate modeling results within specific key study areas of the SASS review. For example, the AB-SRD FMF grizzly bear project, Rattlesnake project, Ferruginous hawk observations, etc.

8) Develop recommendations for spatially–explicit validation study areas, stratified by human use, for long-term biodiversity and rate of change monitoring. I recognize that some of my criticisms may be insurmountable in the short-term. However, I strongly recommend that AB-SRD develop at least 3 reference study areas (high, medium, low human use) where at least the 3-4 species can be spatially modeled to address some of the concerns raised in this review.

9) Develop a spatial model for the SASS study area- A first step towards developing a fully spatial model for the SASS area will be the assembly of spatial layers, for example, the AGCC landsat map mentioned above. With such data, a spatial coarse-filter approach, paralleling that developed in this report, would be a powerful first step towards assessing the importance of spatial relationships for key habitat variables in H.S.I. models. Such a first step might identify that for certain habitats, spatial fragmentation issues are very important, while for others, non-spatial models will suffice. This is a key first step towards developing spatial cumulative effects assessment models.

Finally, I wish to make a general observation, to the credit of the people involved in this report.

Lack of funding, human resources, and management priority- Although this is obviously a difficult topic for a reviewer to comment on, it seemed evident through much of the document that there was not sufficient funding available to produce the very best products for management. There was an acceptance that substandard approaches were necessary as a result. It was acknowledged that results of the expert-opinion based models would not be as defensible as data-derived models, for example, in a court or NRCB hearing about a development project, which makes me question the management utility of the report. Furthermore, I inferred from the duration and long-drawn out nature of the project, which has spanned the period from 1995-the present, that there has been insufficient human resources, funding, and management priority dedicated to the important SASS project. The efforts of the author on this report constitute a major contribution to the understanding and development of a rigorous framework, and I want to make it clear that my critique does not reflect on the people involved in the report. It was obviously a monumental effort, with 100's of people contacted over a period of years. However, to have it all organized by one person when I believe there are sufficient data sets available, for example, to provide some at least limited model validate, belies the lack of management priority and resources dedicated to this project. I believe my review and recommendations make a strong case for increased support, resources, and effort to be directed towards this incredibly important project in southern Alberta.

Reference List

Aldridge, C.L., Boyce, M.S., and Baydack, R.K. 2004. Adaptive Management of Prairie Grouse: How Do We Get There? Wildlife Society Bulletin 32: 92-103.

Beazley, K. and Cardinal, N. Jun. A Systematic Approach for Selecting Focal Species for Conservation in the Forests of Nova Scotia and Maine. Environmental Conservation 31: 91-101.

Bender, L.C., Roloff, G.J., and Haufler, J.B. 1996. Evaluating Confidence Intervals for Habitat Suitability Models. Wildlife Society Bulletin 24: 347-352.

Boyce, M.S., Vernier, P.R., Nielsen, S.E., and Schmiegelow, F.K.A. 2002. Evaluating resource selection functions. Ecological modelling 157: 281-300.

Brooks, R.P. 1997. Improving Habitat Suitability Index Models. Wildlife Society Bulletin 25: 163-167.

Buckmaster, G., Todd, M., Bessie, W., Smith, K., Bonar, R., Beck, B., Beck, J., and Quinlan, R. 1995. Elk (Cervus elaphus) winter range: draft habitat suitability index (HSI) model. In Habitat suitability index models for 35 wildlife species in the Foothills Model Forest. Edited by B. Beck, J. Beck, W. Bessie, R. Bonar, M. Todd, D. Farr, K. Smith, and G. Stenhouse. Foothills Model Forest, Hinton, AB. pp. 51-62.

Burley, J.B. 1989. Multi-Model Habitat Suitability Index Analysis in the Red River Valley. Landscape and Urban Planning 17: 261-280.

Campbell, C., Campbell, I.D., Blyth, C.B., and Mcandrews, J.H. 1994. Bison Extirpation May Have Caused Aspen Expansion in Western Canada. Ecography 17: 360-362.

Carroll, C., Noss, R.F., and Paquet, P.C. 2001. Carnivores as focal species for conservation planning in the Rocky Mountain region. Ecological Applications 11: 961-980.

Crosetto, M., Tarantola, S., and Saltelli, A. 2000. Sensitivity and Uncertainty Analysis in Spatial Modelling Based on Gis. Agriculture Ecosystems & Environment 81: 71-79.

Dettki, H., Lofstrand, R., and Edenius, L. 2003. Modeling Habitat Suitability for Moose in Coastal Northern Sweden: Empirical Vs Process-Oriented Approaches. Ambio 32: 549-556.

Epstein, H.E., Calef, M.P., Walker, M.D., Chapin, F.S., and Starfield, A.M. 2004. Detecting Changes in Arctic Tundra Plant Communities in Response to Warming Over Decadal Time Scales. Global Change Biology 10: 1325-1334.

Geist, V. 1971. The relation of social evolution and dispersal in ungulates during the pleistocene, with emphasis on the old world deer and the genus Bison. Quarternary Research 1: 283-315.

Hastie, T., Tibshirani, T., and Freidman, J. 2001. The elements of statistical learning: data mining, inference, and prediction. Springer, New York.

Hess, G.R. and King, T.J. 2002. Planning Open Spaces for Wildlife I. Selecting Focal Species Using a Delphi Survey Approach. Landscape and Urban Planning 58: 25-40.

Hewitt, C.G. 1921. The conservation of the wild life of Canada. Charles Scribners Sons, New York, NY, USA.

Holthausen, R.S., Widsom, M.J., Pierce, J., Edwards, D.K., and Rowland, M.M. 1994. Using Expert Opinion to Evaluate a Habitat Effectiveness Model for Elk in Western Oregon and Washington. Usda Forest Service Pacific Northwest Research Station Research Paper U1-&.

Johnson, C.J. and Gillingham, M.P. 2004a. Mapping uncertainty: sensitivity of wildlife habitat ratings to expert opinion. Journal of Animal Ecology In Press.

Johnson, C.J. and Gillingham, M.P. 2004b. Predictive accuracy and interpretation of mapped species distribution models. Biological Conservation In Press.

Jones, P.F., Hudson, R.J., and Farr, D.R. 2002. Evaluation of a Winter Habitat Suitability Index Model for Elk in West-Central Alberta. Forest Science 48: 417-425.

Kerley, L.L., Goodrich, J.M., Miquelle, D.G., Smirnov, E.N., Quigley, H.B., and Hornocker, N.G. 2002. Effects of Roads and Human Disturbance on Amur Tigers. Conservation Biology 16: 97-108.

Knapp, A.K., Blair, J.M., Briggs, J.M., Collins, S.L., Hartnett, D.C., Johnson, L.C., and Towne, E.G. 1999. The Keystone Role of Bison in North American Tallgrass Prairie - Bison Increase Habitat Heterogeneity and Alter a Broad Array of Plant, Community, and Ecosystem Processes. Bioscience 49: 39-50.

Krech, S.I. 1999. The Ecological Indian: myth and history. W.W. Norton & Co., New York, NY, USA.

Lehmkuhl, J.F., Kie, J.G., Bender, L.C., Servheen, G., and Nyberg, H. 2001. Evaluating the effects of ecosystem management alternatives on elk, mule deer, and white-tailed deer in the interior Columbia River basin, USA. Forest ecology and mangement 153: 89-104.

Loiselle, B.A., Howell, C.A., Graham, C.H., Goerck, J.M., Brooks, T., Smith, K.G., and Williams, P.H. 2003. Avoiding Pitfalls of Using Species Distribution Models in Conservation Planning. Conservation Biology 17: 1591-1600.

Loukmas, J.J. and Halbrook, R.S. 2001. A Test of the Mink Habitat Suitability Index Model for Riverine Systems. Wildlife Society Bulletin 29: 821-826.

Lyon, J. 1983. Road density models describing habitat effectiveness for elk. Journal of Forestry 22: 592-595.

Mitchell, M.S., Zimmerman, J.W., and Powell, R.A. 2002. Test of a Habitat Suitability Index for Black Bears in the Southern Appalachians. Wildlife Society Bulletin 30: 794-808.

Mysterud, A. and Ims, R.A. 1998. Functional Responses in Habitat Use: Availability Influences Relative Use in Trade-Off Situations. Ecology 79: 1435-1441.

Nielsen, S.E., Boyce, M.S., Stenhouse, G.B., and Munro, R.H. 2002. Modeling grizzly bear habitats in the Yellowhead ecosystem of Alberta: taking autocorrelation seriously. Ursus 12:31-43.

Nielsen, S.E., Boyce, M.S., Stenhouse, G.B., and Munro, R.H.M. 2003. Development and testing of phenologically driven grizzly bear habitat models. Ecoscience 10: 1-10.

Osko, T.J., Hiltz, M.N., Hudson, R.J., and Wasel, S.M. 2004. Moose Habitat Preferences in Response to Changing Availability. Journal of Wildlife Management 68: 576-584.

Roloff, G.J. and Kernohan, B.J. 1999. Evaluating Reliability of Habitat Suitability Index Models. Wildlife Society Bulletin 27: 973-985.

Roloff, G.J., Millspaugh, J.J., Gitzen, R.A., and Brundige, G.C. 2001. Validation Tests of a Spatially Explicit Habitat Effectiveness Model for Rocky Mountain Elk. Journal of Wildlife Management 65: 899-914.

Rosenzweig, M.L. 2003. Reconciliation Ecology and the Future of Species Diversity. Oryx 37: 194-205.

Rosenzweig, M.L. and MacArthur, R.H. 1963. Graphical representation and stability conditions of predator-prey interactions. American Naturalist 97: 209-223.

Rothley, K.D. 2001. Manipulative, Multi-Standard Test of a White-Tailed Deer Habitat Suitability Model. Journal of Wildlife Management 65: 953-963.

Rowland, M.M., Wisdom, M.J., Johnson, B.K., and Kie, J.G. 2000. Elk Distribution and Modeling in Relation to Roads. Journal of Wildlife Management 64: 672-684.

Ruth, T.K., Smith, D.W., Haroldson, M.A., Buotte, P.C., Schwartz, C.C., Quigley, H.B., Cherry, S., Murphy, K.M., Tyers, D., and Frey, K. 2003. Large-Carnivore Response to Recreational Big-Game Hunting Along the Yellowstone National Park and Absaroka-Beartooth Wilderness Boundary. Wildlife Society Bulletin 31: 1150-1161.

Schuster, E.G., Frissell, S.S., Baker, E.E., and Loveless, R.S. 1985. The Delphi Method - Application to Elk Habitat Quality. Usda Forest Service Intermountain Research Station Research Paper 1-32.

Senft, R.L., Coughenour, M.B., Bailey, D.W., Rittenhouse, L.R., Sala, O.E., and Swift, D.M. 1987. Large Herbivore Foraging and Ecological Hierarchies. Bioscience 37: 789-&.

Store, R. and Jokimaki, J. 2003. A Gis-Based Multi-Scale Approach to Habitat Suitability Modeling. Ecological Modelling 169: 1-15.

Timoney, K. and Lee, P. 2001. Environmental Management in Resource-Rich Alberta, Canada: First World Jurisdiction, Third World Analogue? Journal of Environmental Management 63: 387-405.

Truett, J. 1996. Bison and Elk in the American Southwest: in Search of the Pristine. Environmental Management 20: 195-206.

Turner, M.G., Gardner, R.H., and O'Neill, R.V. 2001. Landscape ecology in theory and practice: pattern and process. Springer-Verlag, NY, NY.

Vadas, R.L. and Orth, D.J. 2001. Formulation of Habitat Suitability Models for Stream Fish Guilds: Do the Standard Methods Work? Transactions of the American Fisheries Society 130: 217-235.

White, C.A., Langemann, E.G., Gates, C.C., Kay, C.E., Shury, T., and Hurd, T.E. 2001. Plains bison restoration in the Canadian Rocky Mountains? Ecological and management considerations. In Crossing boundaries to manage wildlife: Proceedings of the 11th

Conference on Research and Resource Management in Parks and on Public Lands, Edited by D. Harmon. The George Wright Society.
Wisdom, M.J. and Mills, L.S. 1997. Sensitivity analysis to guide population recovery: prairie-chickens as an example. Journal of Wildlife Management 61: 302-312.

Review of: Southern Alberta Regional Strategy: Invasion of non-native plant species, report of workshop results.

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1.0 Premise:

The information contained in the report is specific for the ALCES model, which is at a regional scale using generalized coefficients. It is not designed to examine processes leading to the development of a "footprint". The classification of invasive species will be accepted without challenge although *Poa pratensis* is circumpolar and the species in North America also includes native genotypes, which can distort coefficients of invasion. The same may apply to *Bromus inermis*, which might be confused with *Bromus pumpellianus*. Consequently, their presence may relate more to infilling than invasion.

2.0 Workshop Objectives:

-Review current information on invasion of non-native plants into native vegetation types;
-Define groups of invasive non-native plant species which should be modeled;
-Identify land use footprints defined in ALCES that create opportunities for invasion of non-native plants into native vegetation types;
-For each invasive species group, define how ALCES should model invasion from each land use footprint into each native vegetation type.

3.0 Detailed comments:

3.1. This section is based on anecdotal evidence, which confirms that introduced grass species are widely dispersed. I agree that mesic soils are more vulnerable to invasion than xeric soils. However, there seems to be an interaction with plant form and soil type. For example, crested wheatgrass, a tufted species, is more competitive than the native species in the dark brown soils but not in the brown or black soils (for different reasons).

3.2. Estimates on the rates of invasion by alien species are based on an educated guess given the absence of well documented observations. For the same reason, those estimates (Table 3) can not be challenged. Pooling all "invasive agronomics" into a single category doesn't consider the mechanism for invasion, which will affect their rate potential ie tufted (seed) vs rhizomatous (rhizomes and seed), with the former depending on extraneous influences and infilling while the latter is independent of that. While pooling the species is understandable for this exercise, I am puzzled on why different rates were used for different types of linear disturbances. I assume this relates to studies such as Tyser and Worley (1992) who suggest that roadsides were seeded to invasive agronomics while trails were not. Thus, invasive agronomics are established on disturbed soil and maintained by continued disturbances (ie road maintenance) while establishment near trails is accidental and not maintained. The model considers all trails and roadways having a constant invasion factor. This seems to be based on the probability that roadways were seeded while trails were generally not. My assumption needs to be verified.

3.3. I agree that restoration to native vegetation is more likely to occur on Brown soils than on more mesic types. This section details personal observations that aren't disputed.

4.0

4.1 I agree that invasive agronomics (group 1) are easier to model than groups 2 and 3, although group 2 contains some of the more problematic weeds on specific rangeland sites.

The arguments for modelling invasive agronomics are probably correct. I would agree that this group has been given more attention than the others. In terms of understanding their invasion/dispersal rates, however, I suspect more information exists for those weeds that have been the subject of research for control – particularly with biocontrol agents. There is no good data on the invasion of invasive agronomics; most of it depends on a linear interpretation from an establishment period (usually based on seeding) to the present. For tufted species, invasion would occur in pulses when several conditions need to be present to enable invasion.

4.2. I agree, a corridor approach is not logical if the species is invasive. I don't agree that invasive rates decrease over time. They will change depending on the circumstances ie disturbance, soil moisture, timing of precipitation, etc. The suggestion that the rates change supports evidence that the primary cause of early "invasion" is from seeding. Seeding is along a corridor; therefore, a 2-stage approach may be more realistic for roadways than a higher invasion rate constant ie a seeded corridor from which invasion occurs.

4.3. There is very little information documented for invasion of aliens from any source. The mechanism for invasion is the same whether from linear or point disturbances and, therefore, equally predictable. Invasion of tufted species involves an establishment phase (where propagules are dispersed and establish) and an infilling phase. Rhizomatous species invade relative to site conditions ie soil moisture. Neither results in a linear invasion pattern but for this model that is the most feasible approach.

4.4. Pre-1975. Crested wheatgrass was extensively used for pasture reclamation since the 1930's but perhaps not for pipeline reclamation. I think the participants of the workshop have better information on this than I have.

5.0. Here, and elsewhere, the term rates are used in conjunction with time or type of disturbance. I interpreted it to distinguish between whether or not invasive species were seeded deliberately or accidentally. If that is the case, then the term "rates" needs to be qualified. On the other hand, if it actually describes the "rate" from an existing introduction, then rates should be the same regardless of time or disturbance type.

5.1. The invasive component of the model uses general coefficients of invasion to predict the end result of a complex ecosystem process. It illustrates an ecosystem process but it can not predict an outcome. Given the lack of data, a best guess is all that can be applied at this time. This component is not a predictive tool and, perhaps, wasn't meant to be. Its predictions have no credibility despite the use of conservative invasion rate coefficients. A prediction is equally wrong whether it overestimates or underestimates an outcome.

5th item in this section – p 9. Invasive agronomics do not generally invade disturbed sites unless they were seeded. Moisture conditions dictate the success of rhizomatous species. Good condition grassland favours rhizomatous species, whether they are native or invasive, at the expense of tufted species. This occurs because the litter load is greater and enhances moisture conditions.

Summary and suggestions:

- Is the approach/methods used sound? Yes, given the paucity of data; see suggestion for refinement below.

- Are assumptions valid and/or justifiable in light of the information available? Yes, in light of the little information that is available.

- Are data used in an appropriate way? Yes, the data is primarily experiential and can't be challenged.

- Does the module address critical aspects of the module subject area? It addresses agronomic invaders only because they are the most easily modelled. It does not address the noxious invasive weeds whose effect can be catastrophic to an ecosystem. See also comments in 4.1.

- Does the module address critical issues for strategic planning in southern Alberta? Yes, it addresses them but can not make a credible prediction – again due to the paucity of data.

The invasive plant component of the model is based on extremely broad rate coefficients that are sensitized by site, time and type of disturbance, and invasive group type; and derived from a best guess by experienced observers. These attributes are acknowledged with the understanding that changes will be made as new information becomes available – and that background invasion will be captured by a range health assessment component. That seems to be a reasonable approach given the lack of data on the problem. However, this raises the question of purpose for this model component and whether that can be achieved. In its present development, I suggest it is a tool to illustrate a problem and should not be applied to predict an outcome.

An alternative approach (or perhaps the next generation) is to introduce probabilities and partitions. I suggest the credibility of the model would be enhanced by modeling the invasion by species. This would recognize different mechanisms for introduction/invasion and might be developed as follows:

- for each disturbance (linear or point), determine the probability that a species was seeded (whether with a seeder along a road or broadcast from a rider on horseback on a trail – or whatever); the probability could be based on time, type and location of disturbance;

- for each species, determine a coefficient of dispersal and a coefficient of infilling – again for time, type, and location of disturbance;

- since tufted species are mostly dependent on wind for seed dispersal, the direction from the disturbance, in relation to the prevailing winds, would be another factor to consider; therefore, crested wheatgrass would have a greater coefficient of dispersal on the east side of a road than on the west since the prevailing winds are from the west;

- seeded roadways could be treated as corridors and invasion from there would be constant (subject to constraints of probabilities); I think the current proposal already includes the notion of probability by assigning smaller coefficients to trails than roads; the current suggestion would begin with a maximum coefficient for a category and then adjusted by a probability factor;

- assigning probabilities would make it easier to incorporate the effects of disturbances (grazing, fire or protection) on invasion and include other species other than invasive agronomics, which might by invasive species;

Other suggestions for the report:

- Identify the purpose of the model.
- Explain why coefficients are different between roads and trails.
- Identify crucial data that needs to be assembled in order to improve predictions.

Review of: Modeling a Rangeland Plant Community Structural Index In Southern Alberta using ALCES Southern Alberta Sustainability Strategy (SASS)

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Overview:

1. Developmental curves for vegetation types are logical, although the curves for the two fescue grasslands might be altered slightly. Foothills fescue grassland should approach decadence and decline in structure at a younger age than a parkland fescue grassland because of the higher precipitation received in the foothills. For strategic level modeling the difference between the two fescue grasslands might be negligible.

2. The development of structure along the grassland/forest interface is much more complex than is indicated by the developmental graphs. For example, an increase in structural development in a grass-shrub community will usually succeed to a higher shrub cover and a lower grass cover. Shrub structure will succeed while grassland structure will retrogress. An overall increase in grass-shrub structure masks the reality that one segment of the community will advance while the other contracts. This is also true for the forest-grassland interface, but the rate will be slower.

3. Years to recovery of the various rangeland communities is also logical, but is too simple. Emerging theories in rangeland ecology point to a state and transition form of succession rather than a linear progression to a final and definable structure. The state and transition theories seem to be more applicable and the linear succession models less applicable in the rangeland types that are more mesic (fescue grassland types, shrubland transition, and forest transition). In mesic rangeland communities there is a strong possibility that a severe disturbance will result in the community never returning to its original structure. The likelihood of a state change caused by a severe disturbance will be affected by such things as proximity to invasive plants (fragmentation), the severity of the disturbance, the type of disturbance, and the current developmental stage of the community. More advanced communities might be more susceptible to state altering disturbance than less advanced communities. All of these, although complex, could be modeled. The linear models perform better in dry range types than in moist range types.

The linear models are adequate for Mixed Grass Prairie, but have some problems when they are applied to more moist rangelands.

4. Fire is neither constant nor random, and the assumption that all fires set a plant community back to its earliest seral stage is not correct. It is also difficult to believe that an average burn area of 10% (10 year fire return interval) will reduce a grassland community to its lowest seral stage, especially since the recovery time listed for most communities is less than 10 years. The effect of fire on grassland succession and recovery is modeled to be more severe than it actually is.

5. The effects of precipitation on rangeland succession as portrayed in ALCES is logical in the realm of decreased precipitation (drought) but is probably not logical for prolonged periods of above average precipitation. Prolonged periods of increased precipitation might result in "switching" of rangeland communities. For example, a wet period might result in a fescue grassland becoming a shrub or forest-grassland interface. This will not only result in a change in community structure, but a complete change in the community. A prolonged wet period will also speed the recovery of communities that are in lower seral stages, as well as speed the rate and severity of decadence in communities that are in their most advanced seral stages.

6. The effect of livestock stocking on rangeland succession is understated. If a doubling of livestock stocking rate over the recommended will only result in a 10% decrease in community development, then the recommended stocking rate is probably too low. The stocking rate numbers and their relation to community succession sends incorrect messages to policy makers, resource managers, and resource users (ranchers).

Overall Assessment

The ALCES rangeland module is only useful for the broadest planning level, or for presentation to groups or individuals that do not have background in rangeland ecology and management. The developmental succession graphs for the rangeland types are logical and could be applied at the strategic level of planning. The effects of below average precipitation is also logical, but the model will probably underestimate the effects of above average precipitation. The effect of fire on rangeland vegetation appears to be over-estimated and over-simplified. The effects of stocking rate seems to be underestimated.

Unless I am missing something, processes and developmental ecology of rangeland is given a fairly simple treatment. Non-linear relationships are assumed to be linear and there appears to be no attempt to model some predictable events (such as agricultural fragmentation resulting in increased weed invasions). I am a little concerned that presentation of the model results to a knowledgeable audience might result in challenges to the good things the model can predict. Simplicity is a dangerous line to walk on. ALCES seems useful for presentation to broad level audiences that are not expecting detailed analyses and prediction.

Review of: Modeling a Rangeland Plant Community Structural Index In Southern Alberta using ALCES Southern Alberta Sustainability Strategy (SASS)

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I have had a chance to review the range module in the SASS as requested. Let me begin by saying that I think the overall idea of tracking cumulative (and interactive) responses in our rangeland agro-ecosystems is an important one. However, I do have difficulties with trying to represent this process in large models. In particular, my concern is that during this process (1) we are dealing with very complex systems, the complexity of which is typically grossly under-represented by these models, and (2) the ability to model vegetation responses at any scale, including the landscape and plant community level, is strongly limited by the quality of scientific information currently available on how these factors respond to disturbance [e.g. levels of resistance or resilience - rates of recovery)]. The latter problem then leads us to make many assumptions, which are often never tested and potentially result in compounding errors. If we keep these limitations in mind while building these models, what they ultimately may show us is where the greatest deficiencies are in our knowledge base regarding rangelands and their sustainable management.

Here are some more specific thoughts:

Introduction:

1. I sensed some confusion over terminology. For example, I think it would be useful to distinguish between the impact of environmental factors society can't control (e.g. climate, soils, and parent material/topography), and those we can (e.g. the presence/absence of grazing and fire, as well as the frequency, intensity, etc. of both). Range site (or ecosite) is the interaction of the first 3, while the actual plant community (or PC type) is determined by the interaction of range site with disturbances. Because we are concerned with monitoring and evaluating the impact of the latter, this is an easy way to establish a framework for what the model is trying to do, yet this does not seem to come across clearly in the intro.

2. Second, also in the introduction, why is there reference to "patches". Patches may or may not be different communities, depending on the degree of change in plant species composition. I tend to think of patches as an "animal foraging" behavioral construct, not as a discrete tool to map plant community types. Moreover, while over-utilization for one year may result in an "apparent structural change" in vegetation, it may not be indicative of a change in actual range condition or the plant community, as reflected by plant species composition, plant vigor, and most importantly, site stability or protection.

3. The last comment I have based on the introduction is the general use of structure rather than other range condition measures. While I appreciate that structure is a variable that changes

rapidly in response to disturbance and thus, may be a seemingly useful indicator, this same characteristic makes it a problematic one to use ... in that it can show superficial changes. In other words, there is an underlying assumption here that (1) structure is always an indicator of range health and sustainability relative to disturbance, and (2) that structure is a detectable parameter in most rangelands. I am not necessarily convinced of this, particularly in relation to the point I make in #2 above. You need to convince the reader/model user of this.

Study Area

1. The mix of different natural regions makes this a very difficult model to put together, particularly because, as noted above, we often have very poor information on the response of many of these communities to disturbance, etc.

Methods

1. I think you need to explain further the last sentence in the 2nd paragraph, regarding how the rangeland seral development stage index was assigned to cover types. I recognize that you refer to Adams (Barry Adams) as a source, but this is a very important step in the model that needs more information.

2. The community types listed in Table 1 are in some cases, proto-typical (idealized) communities that are well recognized (e.g. the Stipa-Bouteloua (labeled as NTG DMG) grassland association - cv. Coupland). However, in other situations, your community types are not really communities but rather landscape assemblages of communities (e.g. prairie/trees), which may contain many different communities. In essence, Table 1 contains a list of vegetation descriptors, some of which are actual communities (which I would expect to increase or decrease), some of which are community assemblages (which I would expect to change, i.e. to shift with respect their ratio of grassland:woodland). In other words, there are different processes operating at different scales in each of the 'types' you list. Can this be reconciled?

3. Is stocking rate being used as a surrogate for range condition? If so, this is not a safe assumption, as range condition will have a delayed response to accumulative stocking rate impacts over time, depending on the resistance of the plant community. Moreover, when SR does affect seral stage, what are the rates of change for different communities, and why select those?

Sec. 2.3

1. How were the structural index curves developed for the 11 types? While I greatly respect the knowledge of our SRD/public land specialists in the province, it my opinion there is not the data to develop these curves reliably (at least to the point that the larger model will be reliable). At the very least, you need to describe how these were developed, including what info./criteria were used to establish these curves. Finally, you should also discuss the limitations of the data/inputs, as well as the various assumptions made in putting these data together.

2. There are numerous assumptions made regarding the model inputs. Is this the best information that we have? For example, the full recovery rates in DMG of 9 years seem much faster than those that have been documented in some DMG systems by Willms and Dormaar – have these individuals been consulted in establishing these recovery rates? On a more general note, what types of data (including actual studies) have been used in developing your rate

functions in the model? Or were they developed with expert knowledge rather than data? This should be clearly explained in the document.

3. I am not sure that the effects of multiple disturbance on seral stages are additive. In many cases (e.g. grazing X drought) I suspect it is multiplicative (compounding), while in others (e.g. drought and fire) the impacts may be less than additive, depending on the timing of fire and whether the drought occurs before or after the fire. Again, my concern in a more general sense is whether we are over-simplifying these relationships/responses rather than looking at how these factors may interact, which may be more appropriate given the high likelihood that they actually coincide.

4. The last paragraph on pg 4 indicates model responses are evident immediately following disturbance. I again am not sure I agree with this. In mesic systems, degradation may occur quite slowly due to a high inherent resistance to disturbance, but then accelerate past a certain threshold as the system rapidly collapses. Conversely, in arid systems, degradation may occur quite quickly, but the recovery may be very slow. A sensitivity analysis to different time lags (degradation and recovery) and the pt. in #3 above may be useful here to point out important deficiencies in our knowledge/understanding of rangeland vegetation dynamics.

Sec. 2.4

1. How was the "hypothetical" relationship developed between the area burned and degree of change in the community? Is this based on scientific evidence/data from some other studies, or expert opinion?

Sec. 2.5

The quantitative drought responses you describe assume no drought tolerance/resistance, which is not true. Many rangeland systems have considerable resistance initially to individual, short-term disturbances, but when accumulated through time, or when compounded by other disturbances, may exhibit strongly non-linear (and non-additive) responses. Adding this ability would greatly strengthen your model. While this data may be available for some inputs in your model, for others it will be clearly lacking, though I would think this is an important purpose of the model - to test the theoretical interactions among these.

Sec. 2.6

I wonder why, if the stocking rate is 2X that recommended, the plant community is expected to maintain itself at 90% of "climax".... is this in perpetuity even if this grazing level continues for 10 years? This seems to under-cut our current provincial SR guideline. Moreover, if this is the case, what does this say about the grazing guidelines developed and imposed by Public Lands many years ago. Are they recommending far too conservative a SR? Where did this come from?

Taken collectively, I hope my comments are of some use. I have obviously tried to be as thorough and "critical" as possible in the hopes of pushing the developers and users of the SASS model, and to make sure that your model is not a "theoretical house of cards", but rather based on well thought out assumptions, with the best data available. In general I tend to be quite skeptical of modeling, primarily because I believe models are meant to identify questions, not answer questions outright. To that end, I think that if the model is developed using the best information available, and clearly expresses its assumptions and/or limitations, it can be a useful tool for those concerned with the management and conservation of rangelands in southern Alberta.

Review of: Modeling Natural Disturbance in Southern Alberta Using ALCES Southern Alberta Sustainability Strategy (SASS)

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Modeling Natural Disturbance in Southern Alberta using ALCES SASS: Evaluation of Modeling Approach

Since we cannot see the future of how a landscape will change, any evaluation of a program like ALCES depends on:

1. examination of the simulation's structure and the validity of the rate equations being used, and

2. error accumulation and sensitivity of various modules within the simulation. The latter should match the quality of the data being used.

The material at hand (five pages) does not contain enough information to adequately address these two points. The following is my evaluation of the Natural Disturbance module for ALCES based on the limited information I have been given.

First, STELLA seems an inappropriate simulation tool for this application. STELLA decides the relationships (i.e. rate equations) between 'stocks' in the simulation. This is in contrast to typical simulation techniques which require precise definition and greater control of the functional relationships between 'stocks.' Although the goal of this project is not novel or new, there seems to be an attempt to 'reinvent the wheel.' The professional peer-reviewed literature contains a large number of models that have been extensively paramertized, validated, and applied to natural landscapes (see examples in Mladenoff and Baker 2001).

Second, the methodology used in this natural disturbance module is sketchy at best and not presented in a clear or logical manner. I consequently address the appropriateness of the approach and assumptions being used in each section:

Section 1 Two approaches are used for incorporating fire into forests and grasslands: constant proportion and 'random' proportion of area burned. The rationale for using exponential and lognormal distributions is not clear. The constant proportion will produce a negative exponential cumulative age distribution (i.e. time since last burned). The 'random' proportion is determined using a Monte Carlo technique to randomly draw from either an exponential or lognormal distribution. An exponential function could produce extreme value or gamma age distributions.

I am not sure what type of age distributions would result from the lognormal, and I am unaware of any peer-reviewed publications which use lognormal landscape fire frequencies. Also note that the statement “whose mean is defined by an average rate” is equivalent to the statement “whose mean is defined by a mean!”

Table 1 seems reasonable, although one wonders how parkland and fescue values were obtained since most of this area has been cleared for agriculture. Is 69 years a meaningful or reliable value for parkland and fescue landscapes? For grasslands, one would imagine values more on the order of 10 years. All of the values lack standard errors and give a false sense of the accuracy of the return times (e.g. 108 years versus 110 years). This begs the question is the forested areas statistically different from each other?

It is not clear how the information in Table 1a is used in the simulation. For all practical purposes the forest values are the same (approximately 1% per year) and only the grassland value (0.5 % per year) is different. These values give return times of 100 years for forests and 50 years for grasslands. It is not clear why there is a discrepancy between the data in Tables 1 and 1a.

Section 2 Here it is assumed that a fire event will ‘reset’ the area to the youngest of 10 age groups (seral stages). It is not clear how or why the successional pathways or seral stages are divided into 10 age groups. At what ages do stage transitions occur? How was the succession data obtained and how valid is it?

Section 3 This approach seems reasonable if we assume that fire suppression results in a reduction in area burned.

Section 4 What is ‘Cooling the Forest?’ This apparently refers to some process which ‘resets’ the forest to a younger age dominated by deciduous (i.e. aspen) trees which are believed to not burn as easily. I am unaware of any peer-reviewed literature which provides evidence for this phenomenon.

It is unclear how ‘Cooling the Forest’ is incorporated into the simulation. As described in Section 1, annual fire events are modeled using either a constant proportion of fire risk or in the Monte Carlo technique an exponential or lognormal distribution of risks. The constant risk is independent of forest age. Using the Monte Carlo technique, the exponential distribution of risk will increasing with age and the lognormal distribution of risk will peak at a young age and then decrease with age. Without the governing rate equations and framework which structures those equations it is difficult to understand or evaluate this modeling approach.

Section 5 The title of this section is very confusing. It is unclear how fire size distributions are used in the simulation. The distribution used, however, has been well established in the literature.

Where and how does this fit into the simulation? Why a user would be interested in knowing the “number of years required for the pyrogenic edge to become undetectable” is not clear. Here, ‘undetectable’ presumably refers to the inability to detect differences in tree height using air photos. Is this a part of wood volume module (section 6), or is this of interest for something else? Doesn’t the successional sequence also control this? The fire area perimeter data in Table 3 seems to follow the expected power law relationship of area to perimeter! So what?

Section 6 What is the basis of these calculations of wood volume? Is there any empirical evidence to support this approach?

Section 7 It is unclear whether monthly, seasonal, or annual averages are being used. Since the relationships between climate and fire events are not defined here it is impossible to evaluate this approach?

In summary, there is not enough information (i.e. functional relationships, modeling structure, sensitivity analyses, etc.) to adequately evaluate this Natural Disturbance Module for ALCES. The 5 page report is not a very informative or professional report.

Literature Cited:

Mladenoff, DJ, and WL Baker. 2001. Spatial Modeling of Forest Landscape Change: Approaches and Applications. Cambridge University Press, Cambridge, UK.

Compiler's Summary and Comments

Introduction

The process of identifying and engaging appropriate reviewers for this exercise was challenging. The individuals who provided the above reviews are clearly experts in their fields and most have considerable professional and research experience in modeling. However, due to the unique and comprehensive scope of ALCES®, both in terms of scale and integration, it is challenging for reviewers to comment on their respective module within the greater context of the strategic modeling exercise. The intent here not meant to diminish or question the quality of reviews provided above, only to indicate the challenging nature of the task.

Although the sub-models should be valid and reliable on their own, understanding the overall function of ALCES® and how it is being used in the SASS (context) would be beneficial in reviewing the modules, yet was not emphasized significantly in the instructions to reviewers. For example, all of the reviewers raised questions concerning terminology and overall program goals/objectives that were not specifically addressed in the sub-model documentation. Future documentation for sub-models should be developed to be more 'stand alone' (i.e. assumptions and terms explicitly defined) and contain, or be accompanied by, a thorough overview of the use of ALCES® in SASS. In retrospect, it may have been useful to have reviewers attend a one-day workshop on the overall aims and methods of SASS and the use of ALCES®.

Furthermore, there are few other models like ALCES® and, concomitantly, few individuals with experience in evaluating such a model. As Hudson (2002, v; see also, Salmo Consulting *et al.* 2001) concludes:

> ALCES is a good model for this purpose [regional resource strategic planning] and fills a niche shared by few if any alternatives. There are many excellent models for forest, watershed and airshed management. However, they do not work at the same scope and scale.

The compiler recognizes that reviewers were not asked to evaluate ALCES® *per se*, but understanding the overall method and logic of the approach is an essential element of providing a comprehensive review. It would be very useful to solicit a comprehensive review of the ALCES® approach being used to support SASS to complement the module reviews reported herein.

The availability and quality of data was raised by all reviewers. This is certainly not an issue limited to ALCES® and is, in fact, a central concern in all modeling. Where specific data gaps or questions were raised by reviewers, they are indicated in the summary sections below. In addition, it is recommended that the parameterization and population of the ALCES® be used as an opportunity to assess data availability and quality. The comprehensive nature of the cumulative effects approach provides an unprecedented opportunity to assess key data gaps, and thus research priorities, for Alberta.

The reports and background material made available for the review of each module varied considerably in extent and complexity. Therefore, each of the module reviews will be addressed separately below. The compiler has provided a list of key questions or issues arising from the reviews. No attempt has been made to edit, validate or corroborate items raised by reviewers. The assumption is that if these queries or issues are raised by experts, there is either a need for clarification or rectification. For more detail please see the complete reviews above.

Wildlife and Biodiversity

The reviewer recognizes the strengths in the Delphi (expert) approach in the selection of focal species, but questions its strength in the development of the habitat suitability index (HSI) models. In particular, the reviewer points out the failure to incorporate measures of uncertainty into the models. The quantitative and validated HSI approach applied to the grizzly bear was deemed sound and appropriate, but the reviewer questioned the potential to lose some of the critical detail in moving to the spatially stratified domain of ALCES®. Recommendations are made for model validation, sensitivity analysis and uncertainty analysis.

Questions and issues to be addressed:

- Consider better consistency between HSI. scores and categorical ratings for both individual species and species groups.

- Explore the incorporation of uncertainty into the HSI models.

- Explain assumptions associated with the historic baseline as it is used to calculate the ‘index of average change’. Does the approach consider the role of First Nations and the differing ecological relationships existing pre-contact? Explain why this may or may not be relevant.

- Revisit the implications of treating change since historic times and future change as equally important (see Table 1 of the review).

- The title of the report implies that the content deals with both wildlife and biodiversity. However, the report focuses exclusively on the species level and then only on terrestrial vertebrates. Consider revising the title.

- Discuss the implications of a non-spatial approach to modeling in ALCES® especially as it relates to using spatial data derived from the grizzly bear sub-model.

- Provide some background on the habitat categories selected for use in ALCES®.

- Explicitly state assumptions being made in the sub-model components.

- Endemism was not listed in the species selection criteria, but was used to justify lack of species inclusion – explain.

- Provide a clearer explanation for the decision not to use Sage Grouse as a focal species.

- Distinguish between expert opinion and published literature wherever possible.

- How were individual HSI models combined for the species groups – additively, cross projects?

Invasion of Non-native Plant Species

The reviewer provided a relatively positive assessment of the sub-model with affirmative responses to the five guiding review questions. The primary concerns relate to the paucity of ‘hard’ data on the rates, mechanisms and other characteristic of invasive species introduction and expansion. The expert-based approach is recognized as particularly useful in this case. The documentation is clear on the sources of professional/experiential opinion. Data are stronger for invasive species occurrence (e.g. presence/absence) than they are for dynamic processes. The reviewer advocates an adaptive management approach whereby the model is continually refined as new information becomes available. In addition, the reviewer provides suggestions for the development of a more site and species specific model developed through the use of probability factors. The reviewer concludes that the sub-model for use at a strategic planning level, especially for issue identification. The lack of quantitative data makes the predictive capacity of the sub-model less certain.

Questions and issues to be addressed:

- Is there a way to include invasive plant form (e.g. tufted vs. rhizomatous) as a factor of invasiveness? and should invasive agronomics be divided into these two structural groups for greater accuracy?

- Clarify and validate assumptions and rationale for the difference in invasion rates between roads and trails.

- Re-examine the assumption that invasion rates decrease over time. It may be worthwhile considering a 2-stage approach for areas that are first seeded on disturbed ground and then invade surrounding vegetation.

- In considering reclamation practices, the reviewer points out that Crested Wheatgrass was used extensively in pasture reclamation. Should this be incorporated into the sub-model?

- Clarify and be consistent with the term ‘rates’ throughout the documentation.

- Caution should be exercised in using the model for predictive purposes.

- The model fails to address the critical subject o noxious invasive weeds whose effects can be highly detrimental to native ecological systems.

- The reviewer proffers an alternative modeling approach based on probabilities related to site and species, coupled with environmental conditions. The approach is recognized as being more detailed and species-specific.

- It would be useful to include a list of data gaps in the documentation.

Natural Disturbance (fire and insects)

The review of the natural disturbance module was highly critical of both the approach and the parameterization. However, the reviewer clearly states that the documentation provided is not adequate to provide a comprehensive review nor to respond fully to the questions posed in the terms of reference. There is a lack of transparency in the assumptions, terminology, functional relationships and parameterization of this module. It is not possible to make any final statement about this sub-model without further refinement of the documentation followed by another review.

Questions and issues to be addressed:

- More comprehensive and explicit documentation of the approach and choice of parameters is required.

- Documentation of the approach should include reference to modeling for similar landscapes published in peer-reviewed and professional literature.

- Documentation for the sub-model should provide further explanation of the rationale and implications for the selection of constant (deterministic) or random (stochastic) occurrence of disturbance.

- Are there examples of lognormal landscape fire frequencies in the published modeling literature?

- Clarify the meaning of the phrase: "whose mean is defined by average rate" in describing the function of the stochastic mode.

- Does the fire return interval of 69 years reported for parkland and fescue regions (Table 1) refer only to forested components or is this a value for the grassland portions as well? Value seems low (short) for grasslands.

- Provide greater justification for fire frequency values.

- Explain apparent discrepancy between Tables 1 and 1a. For example, if the fire return interval is 69 years for the foothills fescue (Table 1) how can there be a value of 0.05% area burned/year for grasslands (i.e. 200 year interval, Table 1a)?

- Better documentation for choice of '10' age classes.

- Provide further explanation of ‘Cooling the Forest’ and explain the process by which this functions in the model. Alternatively, this function can be dropped from the model description as it is not being used in SASS.

- Explain the significance of ‘pyrogenic edge’ in the overall modeling process.

- Provide justification for wood salvage calculations.

- Further explanation of climate change function is required.

- **It is recommended that the natural disturbance module be reviewed again with greater communication between the reviewer and the developer, and/or more explicit documentation of the module development, assumptions and parameters.**

Rangeland Plant Community Structure Dynamics

The rangeland structural dynamics sub-model was reviewed by two rangeland specialists. Both reviews indicated that the model was appropriate for strategic-level planning and that the approach was sound. Many of the issues raised by the reviewers reflected a need for better explanation of the derivation of model parameters. The reviewers both raised questions regarding model assumptions and the details of using specific parameters (see below). The concern was that the sub-model may be over-general if were to be in applied in more site-specific predictive cases.

Questions and issues to be addressed:

- Describe the methods and data used to develop the structural index curves.

- Should fescue developmental curves reflect earlier decadence and decline for foothills versus parkland fescue?

- Include some discussion of the relationship between the structural index and range conditions.

- Clarify the use of the word “patch” (e.g. as a landscape ecology term rather than as it is used in foraging theory)

- Provide more detail on the statement: “A rangeland seral (development) stage index also was assigned to each cover type based on regional assessment (Adams, unpubl. data).”

- Does the sub-model adequately capture the process of shrub expansion into grasslands? (e.g. the dynamic forest/grassland interface)

- Address the assumption that structural types will return to their pre-disturbance condition following disturbance (years to recovery table)

- Include references for data that were used to develop recovery rates

- Is it valid to assume that fire always sets a community back to its earliest seral stage?

- Is the assumption that “a rangeland type that receives an average or greater than average amount of annual rainfall will be maintained at its oldest seral stage” valid? Should the model consider state changes for prolonged wet periods?

- Does the sub-model adequately reflect differences between communities with respect to drought tolerance?

- Are the effects of over-stocking underestimated with the current coefficients?

Literature Cited

Alberta Environment. 2003. *A Sustainability Strategy for Southern Alberta*. URL: http://www3.gov.ab.ca/env/regions/southern/strategy.html, accessed 22 December 2004.

Hudson, Robert J. 2002. **An Evaluation of ALCES, A Landscape Cumulative Effects Simulator for use in Integrated Resource Management in Alberta**. Report prepared for the ALCES Review Team. CyberCervus International, New Sarepta AB.

Salmo Consulting, Ursus Ecosystem Management and Gaia Consultants. 2001. **Review of predictive modeling tools for wildlife and fish key indicators in the Wood Buffalo Region**. Report prepared for the Cumulative Environmental Management Association. October 2001.

Documents Provided for Review

Anon. n.d. **Modeling a rangeland plant community structural index in southern Alberta using ALCES**. Southern Alberta Sustainability Strategy (SASS), Alberta Environment, Calgary.

Anon. n.d. **Modeling natural disturbance in southern Alberta using ALCES. Southern Alberta Sustainability Strategy (SASS).** Alberta Environment, Calgary.

Anon. n.d. **Modeling the Impacts of Landscape Change in Southern Alberta using ALCES (A Landscape Cumulative Effects Simulator)**. Alberta Environment, Calgary.

Bradley, C. 2003. **Invasion of non-native plant species: Report of workshop results**. Report prepared for Alberta Environment. Lethbridge, AB.

Nielson, S.E. and M.S. Boyce. 2003. **Grizzly bear habitat selection and mortality coefficients of southern Alberta: Estimates for the Southern Alberta Regional Strategy (SARS)-ALCES Project**. A report for Forem Technologies. University of Alberta, Edmonton.

Schieck, J. 2004. **Modeling wildlife and biodiversity for the Southern Alberta Sustainable Strategy (SASS) (Draft May 2004)**. Alberta Research Council, Vegreville Alberta.

Appendix A. Project Terms of Reference

Schedule B – Description of Services

Agreement No. ________

GOVERNMENT OF ALBERTA

Terms of Reference
External Review of *ALCES* Sub-Models
Southern Alberta Sustainability Strategy (SASS)

Description/Project Purpose

Coordinate an independent, expert, technical peer review of the methodology behind five *ALCES* sub-models, and an assessment of whether they allow for useful and reasonable simulations of natural resources over 50 year time-periods in support of current land and resource management decisions.

Provide a report that describes what was examined, summarizes the findings of the review, presents the individual reviews, and provides supplementary background information.

Background

SASS is a Government of Alberta project that looks at social, economic, and environmental information for southern Alberta to take stock of the regions' current situation and to assess the consequences of potential development scenarios over the next two generations. The purpose of SASS Phase 1 is to identify the key issues that need to be addressed in order to ensure a sustainable future with outcomes that are acceptable to citizens of the region and of the province. Ultimately, the results of Phases 1 and 2 will provide a guide for sustainable development. The study area is shown in the attached map (Appendix ___).

SASS is using *ALCES* (A Landscape Cumulative Effects Simulator) to help understand future regional issues and opportunities concerning the sustainable use of natural resources and the environment. Five *ALCES* sub-models relevant to the southern Alberta landscape have been developed. These are as follows:

1. Rangeland Plant Community Dynamics
2. Wildlife Habitat & Biodiversity
3. Natural Disturbance
4. Water Quantity
5. Agronomic Invasive Species

Accompanying reports describe the methodology behind each of these sub-models (Appendix __).

Project Deliverables

The consultant will perform the following tasks:

1. Work with the SASS project team to develop a list of potential reviewers and a set of instructions for the reviewers. (Sample instructions attached).
2. Contact potential reviewers and select one reviewer for each sub-model.
3. Send a copy of the sub-model documentation to the reviewer along with a formal letter of instructions and an attached sheet describing the specific questions to be addressed.
4. Prepare a brief summary report that includes each reviewer's response.

Methods

1. The reviewer should have no professional stake in the outcome of the review. That is the reviewer must have an 'arms length' relationship with Alberta Environment.
2. In general University based experts should be chosen because they are least vulnerable to negative reactions that might arise from unfavourable reviews.
3. The reviewer must be an expert in the subject area.
4. Attempt to recruit Alberta based academics as reviewers since they would be most familiar with the study area and the critical issues surrounding sustainable development in the area.
5. The reviewers must not have had a close working relationship with any of the authors. This included acting as an academic supervisor for a graduate degree.

Timeline

The review process is expected to take about three months.

Appendix B. Text of Initial Letter Sent to Reviewers

Dear ______________,

I am coordinating a peer-review of new model components that have been developed for ALCES (A Landscape Cumulative Effects Simulator). This third-party, expert review is being conducted on behalf of Alberta Environment for use in the Southern Alberta Sustainability Strategy (http://www3.gov.ab.ca/env/regions/southern/strategy.html). The review is being coordinated through the Miistakis Institute (www.rockies.ca) at the University of Calgary. The Southern Alberta Sustainability Strategy is using ALCES to help understand future regional issues and opportunities concerning the sustainable use of natural resources and the environment. Several new sectors relevant to the southern Alberta landscape have been developed and added to ALCES as follows:

1. Rangeland Plant Community Dynamics
2. Wildlife Habitat & Biodiversity
3. Natural Disturbance (fire)
4. Water Quantity
5. Agronomic Invasive Species

ALCES is a STELLA-based (http://www.iseesystems.com/iqitbjiobum3kxmfbm2za355)/softwares/Education/StellaSoftware.aspx) simulation model developed by Forem Technologies (Brad Stelfox et al. - see: http://www.foremtech.com/products/pr_alces.htm).

I am writing to inquire if you would be interested in reviewing the module developed for _______________. The purpose of the review is examine the validity of ALCES models in providing reasonable simulations of natural resources over 50 year time-periods in support of current land and resource management decisions. Questions to be considered in the review include:

- Is the approach/methods used sound?
- Are assumptions valid and/or justifiable in light of the information available?
- Are data used in an appropriate way?
- Does the module address critical aspects of the module subject area?
- Does the module address critical issues for strategic planning in southern Alberta?

Your role would be to examine the module and prepare a brief summary report. The expert reviews of each of the modules will then be compiled and summarized in a report by me. The expected timeline to complete your review would be approximately 2 months (deadline of October 31st). If you feel you have the time, inclination and expertise to conduct such a review, I will need to know what you would charge for your services. You would be paid as a sub-contractor through the Miistakis Institute at the University of Calgary. I have attached a methodological summary of the module for your initial examination. I would be happy to discuss this with you further, but wanted to initiate the process with an e-mail that you could examine on your own time. Please respond to me at this e-mail address.

Sincerely,
Michael S.Quinn, Ph.D.
Director, Miistakis Institute

Appendix C. Biographies of Reviewers

Edward Bork

Dr. Edward Bork obtained his B.Sc. (Agriculture) and M.Sc. (Wildlife and Range) degrees from the University of Alberta, and a Ph.D. (Range Science) from Utah State University in 1997. Since then, Dr. Bork has been the Rangeland Ecology and Management specialist in the Agricultural, Food and Nutritional Science Department at the University of Alberta, where he is currently Associate Professor. He has a broad research program that has included work on grassland fire ecology, woodland responses to disturbance, agro-forestry, landscape forage production dynamics, applications of remote sensing to rangeland monitoring, and range improvements. In addition to supervising 18 graduate students over the last 5 years, he is responsible for teaching 3 undergraduate courses that form much of the core of the Range and Pasture Management and the Wildlife and Rangeland Resources Management Majors within the B.Sc. Agriculture and B.Sc. Environmental Conservation Science programs, respectively.

Mark Hebblewhite

Mark has been a Ph.D. student at the University of Alberta with Dr. Evelyn Merrill since 2001, and is currently studying elk population dynamics, migration, and conservation in the eastern slopes of Banff National Park centred on the Ya Ha Tinda Ranch. Mark was 1 of 2 of the first Canadians to receive the Canon-National Parks Science Scholarship for the Americas for his PhD research in 2002. Mark received the Bill Shostak wildlife award from the department of Biological Sciences in 2003. Mark has over 10 scientific publications on wolf-elk population dynamics, Park management and conservation, climatic influences on wildlife populations, and statistical sampling applications for wildlife. Mark completed his Masters of Science in 2000 in the Wildlife Biology Program at the University of Montana with Dr. Daniel H. Pletscher and Dr. Paul Paquet studying Wolf and Elk population dynamics in Banff National Park. He completed his undergraduate degree in Pure and Applied Ecology at the University of Guelph in 1995 with Honors, completing an Honors thesis on Moose and White-tailed deer habitat use in Algonquin Provincial Park with Dr. John Theberge. Mark has taught undergraduate level courses in statistics and experimental design, co-taught graduate courses in predator-prey dynamics, and was involved in technology transfer programs in Slovakia and Mongolia. Mark has also worked on a wide array of research such as fisheries ecology, forestry-songbird research, montane songbird research, grizzly and black bear research.

Barry Irving

Barry is the manager of the Rangeland and Wildlife, Crops, Compost, Metabolic, and Feedmill research Units, University of Alberta. Barry holds B.Sc. (Forestry), M.Sc. (Range Management), and PhD. (Plant Science), all from the University of Alberta. Barry's background is in integrated land use, especially on rangeland. Barry is a Professional Agrologist and is an active member of the Society for Range Management, a diverse international group of 4,000 members who are interested in the management of rangeland. In his spare time Barry travels to China to "teach the teacher" about North American grazing management and teaching and extension methods.

Edward A. Johnson

Edward A. Johnson is Professor of Ecology in the Department of Biological Sciences, Director of the Kananaskis Field Stations at the University of Calgary, and the G8 Legacy Chair in Wildlife Ecology. His research interests are biogeosciences, particularly the coupling of physical processes to ecological processes, specifically natural disturbances. He has done fieldwork in Canada, Belize, Sri Lanka, and the United States. He has published extensively in refereed scientific journals and has two books on wildfires, Fire and Vegetation Dynamics (Cambridge University Press) and Forest Fires: Behavior and Ecological Effects (Academic Press), and two forthcoming books on Physical and Ecological Processes in Natural Disturbances (Academic Press) and Ecological Education and Environmental Advocacy (Cambridge University Press). For the last 14 years, he has been part of the Natural Sciences and Engineering Research Council of Canada's Network of Centres of Excellence in Sustainable Forest Management. In 1986, he received the William S. Cooper Award of the Ecological Society of America for research on wildfire effects on boreal forest dynamics. He has been a Visiting Professor at the Universities of New South Wales (Australia), Guelph, Wisconsin, and a Bullard Fellow at Harvard University. He is presently the Editor-in-Chief of the Bulletin of the Ecological Society of America

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Walter Willms

Dr. Willms attended the University of BC where he received both a BS in Forestry and MSc from the Faculty of Forestry. His specialization was wildlife management and the MSc dissertation was on the relationships between forestry practices and deer distribution on Vancouver island. He worked for Agriculture Canada at Kamloops from 1971 to 1974 on a multidisciplinary study investigating potential conflicts between cattle and deer on rangeland. This became the focus of his PhD, which was completed at the U of Alberta. He was a research associate and lecturer in range management at the university until 1982 when he was hired as a research scientist with Agriculture and AgriFood Canada at Lethbridge.

The general area of Dr. Willms research has been on the effects of agriculture on native grassland with emphasis on grazing impacts in the Fescue Prairie. More recent studies have investigated the effects of agronomic practices on the primary production, carbon balance, and soil quality in the Northern Great Plains and reclamation of disturbed sites using native or agronomic grasses and the effects of biodiversity on the process. He was appointed an adjunct professor in the department of Agriculture, Forestry, and Nutritional Science at the Univ. of Alberta and a visiting professor at the Inner Mongolia Agricultural University. He was presented with the Emerald Award for Research in 2001. His research has been published in 77 peer reviewed journal articles.
