A COMMENTARY ON MOOSE HABITAT RESEARCH AND MANAGEMENT

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ABSTRACT: A perspective is offered about habitat research for moose (*Alces alces*) in North America. Our knowledge about habitat does not instruct us about its impact on moose population dynamics nor help us to know how we actually should manage forests for moose alone as well as in the broader context of moose as part of whole wildlife communities at the landscape scale. Recommendations are given to improve the questions asked and the conduct of habitat research.

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Management of moose (*Alces alces*) in North America seems generally to be progressing well, that is, if we base our judgements on abundance of animals. Populations appear to be increasing in size or are stable, only infrequently decreasing, and harvests have increased in recent years (Timmermann and Buss 1995). Nevertheless, how much of this is just accidental good fortune is, unfortunately, an important question. Because moose are herbivores we are tempted to think that habitat is critical to abundance; the influence of predation and other factors notwithstanding, it is difficult to shake the feeling that habitat is somehow important to population dynamics. Yet, on the other hand, it is entirely necessary to ask just what is it that we really know about habitat. Two broad questions have been given little attention and are the focus of this article: 1) Just how does habitat influence population trend? 2) How do we manage habitat for moose in today's realm of multiple forest objectives, values, and scales?

Like much wildlife research, our current efforts on moose and habitat seem not to have evolved much over time. The methodology has become more sophisticated in many cases but the questions are quite stagnant. We still are asking what do moose use, little different than that posed by Peterson (1955) or others long ago (e.g., see Bédard *et al.* 1974). Now, habitat use studies are important, indeed fundamentally so, but we still put such documentation to be the primary objective of research rather than to acquire use data in progress as a stepping stone toward larger questions such as the two posed above. The typical habitat use study only documents the sites or forest characteristics used, perhaps selected, by moose. Do we expect that someone else can then easily take such information and change it into meaningful knowledge about processes (question l above) or forest management actions (question 2 above)? We are very expert at employing the Arcadia principle (Hunter 1989) and there seems no end to documenting habitat use, in yet another location or in conjunction with yet another timber management tool (beyond glyphosate) or new gadget (GPS remote sensing and beyond). From the perspective of population dynamics these sorts of habitat studies are not very productive and may be irrelevant. For managing populations, it behooves us to learn about the mechanism(s) whereby (if) habitat drives population and how we may manipulate it (them), yet when we turn to the population dynamics literature we find little to help us. The call for greater experimental and long-term research (e.g., Boutin 1992, Van Ballenberghe and Ballard

1994) to resolve the question of whether predation regulates or limits moose population, albeit valid and fascinating, does not teach us anything directly about the lever we employ daily across much of moose range - timber harvesting and change in forest structure. How and to what magnitude are the effects of predation, hunting, disease, production, etc., modified by our changes in forest structure and dynamics? Overall, the stature of our thinking about habitat simply pales when compared with that about predation and other population factors (e.g., Messier 1994): habitat when considered at all at the population level is superficial compared with the intriguing detail of the dialogue about predation, etc., and habitat is usually taken to only mean the amount of food. We have several well conducted studies on foraging behaviour and on the effect of moose browsing on plant growth and stand development, but we have not scaled upward from these studies to investigate population dynamics. Interpretation and extrapolation of these studies to forest level changes has been made only indirectly in the Discussion sections of papers, posteriori, rather than being used at the outset to structure a research approach.

My observations above are reminiscent of a broader context. Meslow (1993) noted that the bulk of wildlife management activity centers on the ways in which we divide the resource among the users rather than on how to produce the animals. This is certainly evident in moose research and literature. The recent North American Moose Conference (31st, 1995) is a good example: in the Management section, 9 of 10 papers had nothing to do with habitat, six were solely on harvests; in the Habitat section, only 2 of 7 papers were pertinent to managing habitat (no change in 20 yrs. [Bédard *et al.* 1974]). If the literature is to represent our thinking on the subject, I would be led to infer that the abundant moose that we now have, have come about with little worry about habitat (excepting the ways in which habitat might influence the way moose are harvested). We might then be enticed to extrapolate to also think that larger scale research about habitat from here on would not be necessary. This, I suggest, is false. If we change to larger ecosystem level management (below), the capability of the landscape to support any particular resource will decrease, compared with that possible under a single-resource objective, making for a greater proportionate impact of landscape level habitat variables, relative to all other factors, upon realized resource (moose) densities.

The focus of our attempts to manage habitat is the second issue that needs our active dialogue. Clearly, our attention toward moose is much more than we have given to many other forest species. Yet society is slowly evolving toward large management perspectives, that we should somehow manage the whole forest, not only for timber and particular game species but for all system functions and communities (e.g., Booth *et al.* 1993, Kaufmann *et al.* 1994). That we currently do not really know how to do all this will not stop the incentive to move in this new direction. It is impossible to study all species enough to learn what we suspect we need to know in order to manage them individually. In the future, moose management on the ground might work only if we learn how to fit our objectives and tactical operations into the broader objectives of wildlife communities and forest management. But a problem confronting us, in my view, is that no matter how good the individual studies may have been about habitat use and selection, they lose their value when examined in the context of information needs for current forest level management problems: how to design the management of a multi-resource system for which there are competing interests and tradeoffs. This is just one example of what Baskerville (1994) shows

us to be the norm in forest research, that research is usually mismatched with the real management questions. And this is not a new predicament, for much wildlife research takes place at a level of precision or in a direction that is so very different from the knowledge that we need in order to forecast the consequences about the ways in which we actually change habitat on the ground. The values that are expected from our forest systems are changing yet our research has not kept pace to provide us with the expertise to knowingly manage forests as systems. Meslow (1993) observes correctly that wildlife biologists and administrators have failed to think big (and early enough). New technology to improve the accuracy of our knowledge about habitat use/selection will not change this situation unless the questions being asked and the approaches used are also improved. If we continue our fixation on traditional research about moose, we will definitely fit the criticism of "single-product myopia", an error we are advised to avoid (Parsons 1987).

There are at least two ways in which we might direct our attention to remedy some of the problems I identify with moose and forest management. They are not mutually independent. First, in regions with intensive timber management we need to learn how to quantitatively incorporate wildlife habitat attributes within the framework that timber is managed. The need for such integration was earlier explained by Patch *et al.* (1986). This approach is required regardless of whether we treat moose singly as a functionally key species or begin to mesh them with all others as a managed wildlife community. Strategic management of timber resources occurs over large spatial areas and long time horizons. Contemporary habitat models are mostly descriptive and do not encourage us to forecast how moose population will change over a particular long time horizon according to different options for continually changing spatial arrangement of forest structure. If quality of moose habitat can be translated to appropriate timber characteristics, and if we understand how forests change structurally over time, then it should become possible to identify a window of time over which a plant community (forest class type, stand) is appropriate as habitat for moose, how large an area this is and, projected from current conditions, when in the future it will occur. One form of this approach for forest wildlife has been developed by the New Brunswick Department of Natural Resources and Energy (NBDNRE 1991) and it might be used as a template to build from. If windows of suitable habitat described temporally can be further adjusted according to the spatial requirements at the appropriate scale of resolution for the animal, it should then be possible to design different scenarios, each with different amounts of habitat window coming on line over time according to different patterns of intervention. These forecasts along with the necessary subsequent surveys to learn if real population trend approximates that forecasted for the particular habitat strategy implemented, become the core of the management process; the experiments by which we learn. The key is that our knowledge about habitat use/selection must become scaled to the spatial and temporal periods over which timber and forest management planning occurs. If this does not occur, moose will not influence timber management planning other than as periodic crises and constraints. Even if forest harvesting has been generally beneficial to moose populations heretofore there is no professional value in resting on the premise that moose will do all right simply as an outfall of timber management practices.

Second, we might broaden our outlook and apply some of the concepts and techniques emerging from community, ecosystem, and landscape ecology. While some of the ideas about community and forest system

dynamics are likely not directly applicable to single species, some of the thinking is pertinent. Humans want stability yet population dynamics and ecological systems are inherently variable. Hobbs (1995) proposes that management actions to promote spatial and temporal variability in ungulates may be necessary for systems managed for multiple communities and processes. Can we knowingly link individual species to such dynamic community processes? We see that home range size is scaled allometrically across species (e.g., Harestad and Bunnell 1979; see Holling 1992 for theoretical reasoning). Although home range has often been measured for moose it has been used very little. Can we use spatial extent of moose home range to index landscape productivity? To what extent does home range vary according to different patterns of resource availability? Because of mobility, do heretofore normal changes in forest landscape pattern have nil effect on moose density; instead, prompting only changes in dispersion? What is the threshold of changes in forest structure and pattern necessary to promote changes in moose density? If we choose not to manage forest wildlife as communities and continue to pay attention to only select individual species, e.g., moose, I believe we are obligated to learn if and how dynamics of moose index the structure of the remaining community. As well, given the supposed, broad community-wide responsibilities of wildlife management agencies, for what species and to what degree are there opposing consequences from the landscape-wide, system level operations that may be beneficial for moose? To what magnitude does a community-wide management objective change density and other population parameters for a species compared with that achievable under a single-species format? Instead of the traditional concern for level of moose density, does its variability change temporally according to composition of the animal community and forest-wide structure? If we choose to manage forest wildlife collectively as a product of ecosystem processes and landscape pattern, we clearly will broaden our imagination about research as we pay heed to some of the new, provocative thinking about ungulates with other species (e.g., 2nd Ann. Conf., The Wildl. Soc., 1995, Portland, OR). All of the questions posed above may not be useful in all settings. But they may provide direction, and they can be rejected if they serve to build better questions for management research.

To conclude, we have managed moose habitat more by default than with purpose. There is no evidence that we have moved on the challenges and opportunities identified by Crichton (1988). All this is symptomatic of the general conservative and complacent nature in wildlife management. We must take considerable blame ourselves for the all too often opinion that wildlife serve mostly as constraints upon management of other forest resource values. Of course, a major problem confronting us is that we as a society do not have a clear idea of what it is that we want our forests to look like and how we should use them. But this must not stop us; in fact, the need for leadership on natural resources will become more paramount as environmental issues emanating from a generally finite forest escalate. I interpret Thomas' (1992) perspective about professionalism to mean that we do have a responsibility toward public education, to guide and to help the public seek out credible natural resource management goals. We can lead society to define how the forest should look and what it is we as society want from it. It is within this broader framework of stewardship that productive habitat research for moose can and must be put.

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