

November 25, 1992

Dr. Michael Kilgore
GEIS Project Manager
Minnesota Planning Office
300 Centennial Office Building
658 Cedar Street
St. Paul, MN 55155

Dear Dr. Kilgore:

Attached are the peer review comments for the Wildlife Technical Paper. The reviewers were:

- Mr. Keith McCaffery - Wisconsin Department of Natural Resources
- Dr. Robert Giles - Virginia Polytechnic Institute
- Dr. Robert W. Howe - University of Wisconsin

Also attached is a description of how our GEIS Study Team has responded to the peer reviews for this Technical Paper. Should you have any questions or comments, please advise me.

Respectfully yours,



James A. McNutt
Executive Vice President
and GEIS Project Manager

cc: Doug Parsonson
Alan Ek
Bob Dunn
Advisory Committee Members



State of Wisconsin \ DEPARTMENT OF NATURAL RESOURCES

Northern Wildlife Research
P.O. Box 678
Rhinelander, WI 54601
TELEPHONE 715-365-2632

18 November 1992

Dr. James A. McNutt
Executive Vice President and
GEIS Project Manager
5510 Six Forks Road
Raleigh, NC 27609

Dear Dr. McNutt:

Enclosed is my formal review of the technical paper titled FOREST WILDLIFE, a work product completed in conjunction with the Minnesota Generic Environmental Impact Study.

My submission of this review does not place me under any obligations, legal or otherwise, to the State of Minnesota or Jaakko Poyry Consulting, Inc. on matters pertaining to the GEIS work and end product.

Furthermore, I recognize that this review will become part of the public records of the State of Minnesota, as related to the GEIS project.

Sincerely,


Keith R. McCaffery

cc: Dr. Michael Kilgore - Minnesota



MINNESOTA GENERIC ENVIRONMENTAL IMPACT STUDY
 TECHNICAL PAPER REVIEW SYNOPSIS FORM

FOREST WILDLIFE
 Technical Paper Title

Reviewer details:

Name: KEITH MCCREERY

Date Received: 14 Nov 92

Title/Position: NORTHERN WILDL. RES.

Due Date: 20 Nov 92

Address: WIS. DEP. NAT. RESOUR.
BOX 576
RHINELANDER WI 54501

Paper Component

Rating (circle one)

Literature review	1	2	③	4	5	6
Evaluation of Existing Environment/Conditions	1	2	③	4	5	6
Significant Impacts - Assessment Methodology	1	2	③	4	5	6
Significant Impacts - Assessment Results	1	2	③	4	5	6
Mitigation Measures - Appropriateness	1	2	③	4	5	6
Mitigation Measures - Utility	1	2	③	4	5	6
Overall Technical Quality	1	2	③	4	5	6

Rating System

Adequate, no changes needed	1	
Some concern(s), no changes needed	2	(see attachment)
Moderate concern(s), some changes needed	3	(see attachment)
Major concerns, significant changes needed	4	(see attachment)
No comments	5	
Comments not required	6	

Reviewed by: Keith McCreeery

Date: 18 Nov 92

FOREST WILDLIFE
Keith McCaffery

GENERAL COMMENTS:

1. My review was restricted to a limited number of medium and large mammals and grouse due to time constraints and my scope of expertise.

2. Based on the published schedule, I dutifully defended the last week of October for this review. The document arrived unexpectedly 14 November, the week prior to our deer season when I was committed to be out of the office for three days. This was not good for you or me. The following is the best that I could do in light of circumstances.

3. In my review, I was immediately struck by projected impacts on two key species: snowshoe hares and deer. Hares are clearly favored by aspen clearcutting, especially when in close proximity to lowland conifers. It is difficult for me to reconcile this with the projections that show little or no impact on hares. Meanwhile, the impacts of accelerated timber harvest on certain other mammals (fishers, martens, flying squirrels) are projected to be rather profound. Intuitively, this causes me some dissidence. Fishers (and several other predators) should track with hares. Prey base is more important than specific habitat in that fishers and cats seem to be pretty adaptable. A specific example of another inconsistent linkage between prey and predator can be found on P. 107 when talking of wolves, deer and moose.

4. Increased cutting is projected to negatively affect deer populations. Even the document indicates that this runs counter to conventional wisdom (P.110). The highest deer populations in history in the northern Great Lakes region occurred following catastrophic disturbance of the forest. It was changes in the quantity of summer range, not thermal cover that caused the deer irruptions. I can only speculate that these GEIS projections resulted from over-reliance on pre-1970 literature, overly constrained definition of winter cover, and overrated significance of occasional acorn crops. I believe the projected impacts on both hares (and related species) and deer are wrong.

5. The narrative of the document is riddled with other minor glitches that would certainly have to be corrected if this were to be published as a scientific document. If left uncorrected, the document is going to appear hastily prepared with limited quality control. This will cast doubt on the credibility of the whole. Cumulatively, some of these glitches appear to have resulted in erroneous assumptions that underlie the GEIS analysis. Some examples are provided in the specific comments below.

SPECIFIC COMMENTS (GEIS draft dated November 1992):

P.11 P1: Hare habitat is described, but omits young aspen (5+ years), with its usual Rubus and cherry component, as prime habitat. This may have resulted from over reliance on the Ontario study indicating 20-year-old aspen as best.

P.16 Fisher: Habitat use is closely linked with primary prey: hares.

P.17 Deer: Herds "peaked in the 1950s" like they did in the 1940s in Michigan and Wisconsin primarily because of the cutting and fires that took place in the decades immediately prior. The importance of light penetration to the forage strata of deer is not strongly recognized in this report. You may wish to consult Mautz (1978) and McCaffery (1986).

- P.18 P3: Occasional fir die-off due to budworm does not disqualify it as an important thermal cover for deer. The majority of northern Wisconsin deer (no doubt elsewhere) use fir as winter cover. Fir is an aggressive increaser despite setbacks by insects and cutting.
- P.18 P5: Cool season grasses are important forage in spring and fall.
- P.19 P1: Aspen older than 25 remains good habitat for deer. Branch breakage provides aspen leaves for deer even in vigorous pole stands (Bauer 1977). Breakup of decadent stands results in heavy shrub and herbaceous development.
- P.19 P4: Hemlock is barely present in Minnesota.
- P.19 P6: Reference to deer damage to threatened plants probably alludes to a review paper by Alverson, et al. May want to wait for data, or let this be discussed in Biodiversity document.
- P.20 P1: If I understand ecological trends in MN, there is a trend for more conifers in the northern forest despite cutting. I doubt that removing allowable cut would result in "excessive loss of conifers." This GEIS conclusion may result from constraining thermal cover to sawlog size conifers.
- P.24 P1: The importance of forest openings to bears is overlooked. Grasses, cherries, berries, and ants are important sources of food. This habitat was one of the most heavily used by bears (Kohn 1982).
- P.41 P1: "Little use of conifers" is based heavily on the Cloquet experience. Others have reported differently (Kubisiak, Moulton and McCaffery 1980; Thompson in Missouri).
- P.57 P1, 1., 1a: Excessive weight is placed on mature conifers. Clearly conifers do not have to be sawlog size to provide thermal cover. The vast majority of cedar, fir and mixed conifer swamps used for winter cover are in the pole-size. Cut redpine is a net benefit to deer. Conifer succession is a threat to long term deer range productivity.
- P.58 P1: The systematic underestimation of functional deer cover results in faulty impacts projected from accelerated timber harvest. Ten percent yard cover in most of the Lakes States is probably adequate and it doesn't have to be mature conifers; seems that Verme (1965?) indicated something about 30 feet height.
- P.78 Fig 4.1: Though I have not studied the narrative that is relevant to chipmunks, I immediately question the 1st decade impact of all cutting scenarios. Is the regional response going to be this great? If so, why not hares?
- P.79 Fig 4.4: If enough habitat is going to be affected to cause rather profound effects on many small and medium sized mammals, it seems there should be a significant impact (positive) on hares. There should also be a corresponding impact on predators that depend heavily on hares as prey.
- P.104 Fisher: I didn't notice how forest area was going to be lost. Is this to urbanization and farming? Is the loss projected to be that great as to affect fishers?
- P.107,4.2: None of this seems to make sense to me. If declines are expected in deer and moose, the wolf economy is going to be impaired and should result in a decline in wolf population. Beavers as prey won't totally compensate. Furthermore, deer are more likely to increase as suggested in Append. 3-2. Bears are also favored by timber cutting. The availability of soft mast and other foods is greatly enhanced by forest disturbance. Soft mast is far more reliable than acorns.
- P.108 P1: Appendix 8 was not included with my document. My last page is App. 6.

- P.108 Table 4.8: This may be the main culprit for spurious projections. The definition of adequate thermal cover is overly constrained. The significance of thermal cover for much of northern Minnesota may be overplayed. Severe winters don't occur every year. Losses may be more severe in the absence of good cover when they do occur, but herd recovery is quicker than where there is excessive conifers. Where severe winters do occur regularly, you are looking at moose range anyway.
- P.109 Table 4.9: See comment above.
- P.110 and Table 4.10: I was confused by this presentation. Discussion seems to be about negative trend, while table shows positive impacts.
- P.112, 5: I disagree with conclusion. Deer herd will be benefitted by accelerated cutting.
- P.112 Bear: While bear love acorns, they are clearly not dependent upon them. I fear acorns were over-rated in these analyses. Acorn crops are quite widely spaced geographically and temporally. Bears must survive between times on other foods. Cutting results in increased production of alternative bear foods.
- P.114 Moose: I defer to moose experts.
- P.114 Wolf: If deer decline as report asserts, wolves should decline (even if I don't believe this analysis).
- P.138 Grouse: The conclusion for ruffed grouse seems to run counter to conventional wisdom and historical experience.
- P.169, 10: Definition of lowland conifers is overly constrained. Conifers certainly need not be present on every section of land for deer.
- P.170, 12: Cutting oak and mtn ash can be easily mitigated by not cutting some oak and mtn ash. However, I believe the report over-rates the possible impact on oaks and the effect on many northern wildlife species.
- P.173, 6.3: Silvicultural alternatives are discussed without identifying forest types. Presumably, most cutting is planned in aspen, spruce and fir. Selective cutting does not seem appropriate as a mitigation measure in these types (P.174).
- P.175 Acorns: In addition to reserve trees, individual stands should be identified for mast production and specialized management.
- P.178 Conifers: "Conifers" is a vague term. Pines have quite different roles compared spruces and fir. I can't imagine Kooch county with a shortage of conifers! I have difficulty seeing clearcutting as a threat to upland spruce-fir. In the absence of repeated fires, fir is destined to increase.
- P.180 P3: The concerns for forest interior species may be misplaced. At present there is no clear consensus that clearcutting has significant effects on forest interior species in areas of extensive forest land. Save this debate for the Biodiversity report.
- P.184 Openings: Maintenance of existing relict openings should be given highest priority as a cost effective means for offsetting declining carrying capacity for selected species caused by forest succession. Cost effectiveness of constructing openings is still open to study (Lenarz 1988).
- P.185 P1: This flash of conventional wisdom regarding possible deer population trends resulting from timber harvest is not consistent with the GEIS analysis (P.110).
- P.186 P6: Deer will not benefit from long rotation northern hardwoods (Cf. McCaffery 1976, 1986).

- P.187 Selective: Selective cutting will not favor deer and bear significantly. Conventional selective cutting does not permit adequate light to get to the forest floor. Silviculturally, you do not want to encourage adventitious branching or biomass going into shrub understory. Therefore, forage production is limited.
- P.188 P2: Again, while very important as a source of food when and where available, I believe this report over emphasizes the significance of acorns, especially to deer and bear.
- P.188 P4: I know of no instance where ruffed grouse and woodcock are favored by selective cutting.
- P.191 P1: I disagree with conclusion that conifer mitigation is generally needed. I feel definitions and assumptions used in developing this analysis were overly constrained (See above).
- P.193 Openings: You might include bears as a beneficiary of maintaining relict openings which typically contain cherry and berry producing plants, plus ants and grass.
- P.194 P5: I disagree with the need to deliberately retain conifers in most of northern MN. The ecological trend is for more conifer in this region, not less, despite cutting practices. From the standpoint of game productivity (other than perhaps moose) this trend is going to reduce carrying capacity. I believe that this recommendation is based on faulty assumptions in the analyses.
- App. 2-1 Hare: I agree that clearcuts are little used during the "first few years after clearcutting," but that they are used extensively a few years thereafter. I don't agree that 20-year-old stands are generally prime habitat for hares. Heaviest use appears to occur between 5 and 15 years. It is then that aspen bark is a primary food and Rubus and other shrubs are maximally developed as well.
- App. 2-23 P2: Southernmost fisher range may be in Wisconsin.
- App. 2-25 P2: Cats will follow the hares. When hares are relatively abundant and using young aspen habitats, that is where the cats will also be found.
- App. 3-1 P2: Fawns in southern Minnesota breed in December (Ingebrigtsen 1988). Quadruplets are rare. "Birthing" is unaffected by severe winters, but survival of neonates is profoundly affected (Verme 1962, 1977, etc.). Fawns are dropped in a fairly random fashion irrespective of ground cover.
- App. 3-1 P3: Rogers et al. (1981) summarizes a more contemporary understanding of seasonal deer foods.
- App. 3-1 P4: Winter cover may be "critical," but its nature varies dramatically from the North to the South. It may be cattails in the South. MNDNR (1990) is not listed. Winter cover certainly does not need to be "mature" or "sawlog" conifers.
- App. 3-2 P2: P. tenuis paragraph might better appear in moose section.
- App. 3-2 P4: Overharvesting concern needs discussion. The concern may apply if there is a target age structure for adult bucks. Otherwise, the agency merely reduces antlerless quotas to control deer harvest.
- App. 3-2 P6: Light penetration is what makes some habitats better for deer than others (McCaffery et al. 1974, McCaffery 1986).

App. 3-3 P1: Corn and artificial foods cannot be equated with wild foods. The normal northern winter browse situation is woody twig ends that do not contain sufficient nutrition to maintain body weight (Ullrey et al. 1970). Good summer and fall forage is necessary for fat accretion to survive harsh winters (Mautz 1978).

Yarding types are here ranked for preference (quality?), but did model consider any qualitative differences in types? Were all types considered equally vulnerable to timber harvest?

Tamarack and black spruce are not considered quality winter cover, but may be heavily used in farmland where other cover is lacking.

App. 3-3 P2: Cover need not be in close proximity to regenerating "intolerant" types. Better if yard cover is in close proximity to cut tops or regenerating midtolerant or tolerant types. While birch and other associated hardwoods are pretty good food, aspen twigs are not considered prime winter browse.

App. 3-3 P3: McCaffery et al. (1981) not listed.

App. 3-4 P1: "Permanent" openings need not be adjacent to or near yards. Deer movement to "summer" range from yards may be measured in miles/day.

App. 3-5 P1: Breeding occurs during summer and implantation occurs in December.

App. 3-5 P4: On better bear range, the reproductive cycle is much earlier than the 6.3 reported here. Most sows produce cubs by 3-4 years in better habitat.

Increased access may increase ease of harvest exploitation, but the harvest is controlled by quota permit.

App. 3-5 P5: The bear's adaptability is far more related to the fact that it is an omnivore than its ability to hibernate.

LITERATURE CITED

Bauer, W.A. 1977. Forage preferences of tame deer in the aspen type of northern Michigan. MS Th. Mich. Tech. Univ. Houghton. 85pp.

Ingebrigtsen, D. 1988. Farmland deer productivity. Minn. Dep. Nat. Resour. Intra-Dep. Memo. 7pp.

Kohn, B.E. 1982. Status and management of black bears in Wisconsin. Wis. Dep. Nat. Resour. Tech. Bull. No. 129. 32pp.

Kubisiak, J.F., J.C. Moulton, and K.R. McCaffery. 1980. Ruffed grouse density and habitat relationships in Wisconsin. Wis. Dep. Nat. Resour. Tech. Bull. 118.

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- Verme, L.J. 1965. Swamp conifer deeryards in northern Michigan: their ecology and management. J. For. 63:523-529.
- Verme, L.J. 1977. Assessment of natal mortality in Upper Michigan deer. J. Wildl. Manage. 41(4):700-708.

Keith McCaffery 18 Nov 92
Reviewer Date

The Following Details the Responses to the Review
Forwarded by Keith McCaffery

General Comments:

Points 1 and 2:

The reviewer acknowledges that he only reviewed large mammals, a few medium sized mammals and grouse. No response is required.

Points 3 and Specific Comment on Page 79, Fig. 4.4:

The reviewer comments on two impacts: (3a) snowshoe hare and predatory small mammals. He finds it difficult to reconcile the fact that projections show little impact on hares, even though they should respond favorably to clearcutting of aspen. Also fishers, pine martens and cats should track with hares because prey base is more important than habitat.

The authors point out that no impacts were projected for snowshoe hares; impacts on fisher occur mostly in the last decade or two of the high harvest scenario (revised analyses indicate no statewide impacts on the fisher). Although the bobcat and lynx are projected to experience impacts on an ecoregion basis, there are not significant statewide impacts on these species. Thus, the disparity between modelled impacts on hares and on fisher is not as severe as the reviewer's comments imply, although it clearly exists (see below).

Cats and hares are tightly linked, as the reviewer implies. However, this linkage was difficult to model precisely in this analysis in a way that is meaningful biologically. Although hares are found in a variety of habitats at the peak in the cycle, they are concentrated in lowland conifers during lows in the cycle. As pointed out in the document, this pattern was modelled by focusing the analysis on "bottleneck" habitats: lowland conifers are far more critical habitat for cats than are young aspen stands in this sense. Even though young aspen stands may be important habitat for hares at some times, these stands are relatively unimportant in the linkage between hares and cats at periods during the hare cycle that are difficult for cats. We would miss the biologically crucial patterns by emphasizing only average habitats or only habitats where a species like the snowshoe hare is present at peak abundance. Undoubtedly, the focus employed in the analysis of bobcat and lynx resulted in some (intentional) uncoupling of the average linkage between cats and hares.

Although fishers make extensive use of hares, the linkages between these two species probably are not as tight as between cats and hares. In the analysis, no attempt was made to model a specific dependence of fishers on hares; the impacts observed for fishers do not depend directly on the outcome for hares. There is considerable published evidence (cited in the species accounts in the document) that fishers make little use of early clearcuts, especially in winter, and that they decline in areas where extensive clearcutting has taken place. However, there are some recent MDNR observations, not

communicated to us in MDNR comments on the first draft of the document, that conflict with the published information. The resolution of these conflicts is not clear. However, the observations of fisher use of young aspen stands in Minnesota are in a landscape context that includes substantial mid-aged forest. It is arguable whether use of young stands would be similar in the 4th or 5th decade of the high scenario (when most significant impacts occurred for fishers) when much more of the landscape would be covered with young stands. Furthermore, the reviewer is reminded that the impacts reported for fishers and other species reflect not simply the effects of timber harvest, but also changes over time in forest area and in forest type, as modelled by the Maintaining Forest Productivity and Forest Resource Base Study Group.

In addition, modeled responses of predators and prey are not always expected to be directly linked in the GEIS analysis. It would be far too simplistic a view of the world to expect this outcome in all instances. Clearly, predator abundance affects prey abundance and vice versa. However, some mammals in both categories are affected by weather and climate to differing degrees because of body size and other morphological differences. Herbivorous prey, such as hares, are affected by features of the vegetation, some of which are affected in feedback fashion by the herbivores. Predators and prey differ in their degree of diet selectivity, so that each may be affected differently by variation in food availability. Many predators take a variety of prey, and some, like the fisher generalists that can take squirrels, birds, carrion and hares. This means that many predators will not show a direct correspondence with a single prey species. Also, some species in both predator and prey categories require particular features of habitat structure, that may affect the likelihood that a predator will catch a prey animal or that a prey animal will escape predation. Spatial variation of these components of habitat, of course, weakens the link between predator abundance and prey abundance.

Changes made in the document: None

(3b) impacts on white-tailed deer. This is handled in detail under point 4 below.

Point 4, Also Specific Comment for Page 57 P.1 and Page 58 P. 1, Page 108, Table 4.8:

The reviewer points out that he believes the projections for hare and deer are wrong. The hare was dealt with under point 3 above. Here the authors respond to the reviewer's remarks about deer. The reviewer is correct that deer irruptions in the lake states occurred after heavy region-wide logging, and that the winter conifer aspects of the modeling done by the wildlife study group were too constrained in that only sawtimber sized stands were included. However, the authors still think that winter conifer cover is more important in northern Minnesota than in northern Wisconsin. Mean winter temperatures are 6 to 10 degrees F colder in northern Minnesota than in Wisconsin, and winter starts several weeks earlier as well, so that deer need to generate more body heat to survive, and for a longer period than in northern Wisconsin. Deer in northern Minnesota are near the northern edge of their range.

Changes made to the document: The study group reanalyzed the potential impacts of harvesting on deer, using similar methods in the previous document, but including both pole and sawtimber sized conifer stands.

Point 5:

The reviewer points out in the detail under his specific comments that the document appeared to be hastily prepared, and as such, had numerous technical errors that require correction. The reviewer's point 5 is really an introduction to these specific comments, which are numerous. The specific comments were handled in three groups. Group 1 includes those that relate to one of the general points above and were included in with the appropriate general comment above. Group 2 covers those that raise a significant new point and are responded to below. Group 3 encompassed the reviewer's suggestions for minor wording changes or clarifications that may strengthen the paper; a majority of which were incorporated by the wildlife study group, although not listed specifically here.

Page 11, P 1: The reviewer points out that hare habitat should include young aspen (5+ years). Earlier he mentioned in the General comments (point 3), that young aspen is also good habitat when in close proximity to lowland conifers. However, as was pointed out in the paper, there is no basis in the available data for incorporating such detailed spatial information in the habitat modeling. It is possible that the value of young aspen for hare habitat was underestimated, however, since no significant impact on the hare was detected, this does not seem particularly important.

Changes made to the document: None

Page 19, P. 6: The reviewer states that the relationship of deer to plant diversity should be mentioned in the biodiversity paper instead of here. The authors agree.

Changes made to the document: The paragraph in question has been deleted.

Page 20, P. 1, Page 194, P. 5: The reviewer questions whether increasing the cut in Minnesota would result in excessive loss of conifers. This is discussed in the biodiversity paper, which points out that the situation with respect to conifer retention and harvesting is not clear. There is not a clear trend toward or away from conifers in the last few decades.

Changes made to the document: The work "excessive" has been deleted to make the sentence in question less strident.

Page 24, P. 1: The reviewer states that the importance of openings with berries, etc. is overlooked. However, this is covered in the more detailed appendix description of bears (Appendix 3).

Changes made to the document: None

Page 41, P. 1, Page 138: The reviewer states that in other areas, ruffed grouse apparently use conifers. However, the authors feel it is best to stick with the references already cited.

Changes made to the document: None

Page 78, Fig. 4.1: The reviewer questions the predicted impact of harvesting on least chipmunks. Considerable literature indicates that least chipmunks are closely linked to recent clearcuts. Their use of these habitats is not linked to nearby presence of conifers, as is the case for snowshoe hares, and thus, is more straight forward to model.

Changes made to the document: None

Page 104: The reviewer did not notice how the forest area would be lost. It would be to agriculture in the west and recreational development in ecoregion 3, which would lose at least 7 percent. Ecoregion 4 would see a complex loss to agriculture, recreational development and urbanization of 3 to 7 percent. These figures are estimates from the Maintaining Forest Productivity and the Forest Resource Base Technical paper.

Changes made to the document: A brief listing of projected forest area loss is inserted in section 3.2.2.

Page 107, section 4.2: This paragraph is no longer relevant with the new analyses of deer, bear and moose in the final version of the paper.

Changes made to the document: Section 4.2 has new data and has been heavily edited, with some sections replaced.

Page 112 Bear: The reviewer correctly noted that bear are not totally dependent on acorns.

Changes made to the document: The new discussion of bear makes it clear that acorns are a major factor for bears, but not the only one.

After review by the Advisory Committee, please advise me if there are any significant issues you regard as outstanding. In the absence of such, this paper will be submitted to the EQB for formal approval.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager

November 25, 1992



VIRGINIA POLYTECHNIC INSTITUTE
AND STATE UNIVERSITY

Department of Fisheries and Wildlife Sciences

School of Forestry and Wildlife Resources
College of Agriculture and Life Sciences
Blacksburg, Virginia 24061-0321
(703) 231-5573 Fax: (703) 231-7580

December 3, 1992

Dr. James McNutt
Vice President
Jaakko Pöyry Consulting, Inc.
Raleigh, NC 27609

Attn: Jan Rushing

"Forest Wildlife" is an excellent document and I congratulate your team on its impressive work. My comments have been faxed for use this past weekend. Enclosed are typed notes. Please distribute these to the appropriate parties.

Please call me if there are questions.

Sincerely,

A handwritten signature in black ink that reads "Robert H. Giles, Jr." in a cursive style.

Robert H. Giles, Jr., Ph.D.
Professor, Wildlife Resource Management

MINNESOTA GENERIC ENVIRONMENTAL IMPACT STUDY
TECHNICAL PAPER REVIEW SYNOPSIS FORM

FOREST WILDLIFE
 Technical Paper Title

Reviewer details:

Name: ROBERT H. GILES, JR. Date Received: _____
 Title/Position: PROFESSOR WILDLIFE Due Date: _____
 Address: RESOURCE MANAGEMENT
VPI & SU
BLACKSBURG, VA 24061-0321

<u>Paper Component</u>	<u>Rating (circle one)</u>					
Literature review	①	2	3	4	5	6
Evaluation of Existing Environment/Conditions	1	② → ③	4	5	6	
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Rating System

- | | | |
|--------------------------------------------|---|------------------|
| Adequate, no changes needed | 1 | |
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| Moderate concern(s), some changes needed | ③ | (see attachment) |
| Major concerns, significant changes needed | 4 | (see attachment) |
| No comments | 5 | |
| Comments not required | 6 | |

Reviewed by: Robert H. Giles, Jr. Date: 11-21-92



VIRGINIA POLYTECHNIC INSTITUTE
AND STATE UNIVERSITY

Department of Fisheries and Wildlife Sciences

School of Forestry and Wildlife Resources
College of Agriculture and Life Sciences
Blacksburg, Virginia 24061-0321
(703) 231-5573 Fax: (703) 231-7580

December 3, 1992

Dr. James McNutt
Vice President
Jaakko Pöyry Consulting, Inc.
Raleigh, NC 27609

Dear Dr. McNutt:

Enclosed is my formal review of the technical paper titled Forest Wildlife, a work product completed in conjunction with the Minnesota Generic Environmental Impact Study. This is an excellent document and I find no major flaws. I have made suggestions that I believe will improve the text and improve its longterm usefulness.

My submission of this review does not place me under any obligations, legal or otherwise, to the State of Minnesota or Jaakko Pöyry Consulting, Inc. on matters pertaining to the GEIS work and end product.

Furthermore, I recognize that this review will become part of the public records of the State of Minnesota as related to the GEIS project.

Sincerely,

A handwritten signature in cursive script that reads "Robert H. Giles, Jr.".

Robert H. Giles, Jr., Ph.D.
Professor, Wildlife Resource Management

Notes on "Forest Management"

- p.iii line 4 from bottom - retaining conifers, protecting riparian zones, improving harvest schedules, and altering harvest patterns are judges . . . [I'll comment on why later.]
- Suggest adding idea of simultaneous use of the 13 above is needed . . . but the top 4 are The 4 (only) make it sound too easy.
- p.1 line 2 - "forest management" is so inclusive and covers fire control to taxation. I hope you will consider an expanded few words that narrows your study to what it really includes.
- p.1 mid page - 10 groups; bottom of page - 8 groups?
- p.9 inconsistent use of modeling; page 9 - bottom
- p.11 line 11 - suggest adding . . . and then decline. Implication here is that they stay abundant. [Is the increase to 20 years max and then decline included in the type/age model?]
- p.13 Beaver trapping emphasizes the need to involve demand or use or value in your intro comments. The study is about populations and habits, but pure numbers are as meaningless as cords of wood available without an idea of the price. The study is about a resource with human value, not raw numbers. The point is conspicuous on p.17. The point has extra meaning as your study strategies to mitigate . . . which can then become educational, influencing value, changing use and use rates, etc.
- p.19 Deer increases will influence costs of or success of reforestation in regions. Harvest rates must be assured as in open land (paragraph 5). An increase in deer in some regions may not be "good" unless an assumption of control goes with it.
- p.21 bottom of paragraph 4 - delete purposive comment - observation is ok - behavior results in food being within reach.
- p.31 paragraph 1 - High costs and vast areas makes suggestions of bird houses very weak. I would leave them as an observation -- they influence -- but do not hint that they are a realistic forest wide option.
- p.41 Utilization as well as production is important. Are all of the available birds (surplus) being harvested? Emphasis on production to meet demand of users - even future users - should be included here to make this (and other faunal categories) relevant. More may not be needed (given careful

thought and analysis) even though for most "game" the need for more seems like a truism.

- p.44-45 Mitigation strategies of education, road or area closures, etc. seem suggested here, along with BMP (e.g., p.46 protect small forest wetlands i.e. the spring peeper) or p.46 make frogs illegal for use in classes or taken by quota as with fur trappers).
- p.50 Given USFS "ecosystem management" policy and anti-clearcut sentiment growing and in law and likely influence on future policy, some comment on how this will influence future results seems reasonable. Clear-cut or nothing, even though that is what the data support, needs further comment to be realistic.
- p.51 line 8 - associated may be the word to add to my comment on p.1, line 2.
- p.54 Not clear, but I assume you assigned likely change in abundance in each species in each decade (an animal "yield curve").
- p.59 Careful with net analyses in area. (My feet are dry and my head submerged; net = I should be comfortable!) Same with too many or too few deer.
- p.66 3rd line from bottom - presence or bottom paragraph ??? Presence or loss was computed over 5 decades; % change with abundance data was computed. What is the meaning of p.66, 4 lines from bottom? How related?
- p.77 I am unsure: Isn't it likely that the rattlesnake will increase under the high harvest level re meadow vole and increased edge assertion? This may not be a big increase but since the animal is poisonous it may be worthy of note. (Such increases have happened elsewhere.) The rapid recovery of rodents suggests only short term (not rotation-long) decline with a high harvest level, thus a fertile rodent resource for snakes, foxes, etc. P.52-54 leaves doubt about how changes in the last 4 decades are accounted. I think rapid recovery of rodents after 10 years is common and thus influence over 40 years may dwarf those changes estimated in year 1-10.
- p.103 White-footed mice declined (p.77) under 2 scenarios - inconsistent?
- p.115 I cannot find the source of 79 harmed stated on p.ii It should not be 19 + 60 in Table 415 due to overlaps. (?)
- p.157 Please consider a modified definition: a wildlife impact of a forest harvest or management activity is considered significant if it is estimated to be the major cause of a change in populations (+ or -) or animal space (cf

habitat) so as to cause significant (>25%) undesirable, long-term change in population abundance or their related benefits or disproducts in an ecoregion of the state.

Reductions of over 5% for any habitat or population for species of special concern, or those threatened or endangered (federal or state) are considered significant.

- p.158 line 8 from the bottom - Suggestion: "... increase is not of any presently perceived ecological or economic importance."
- p.167 My only major insecurity about the work described is the lack of attention to age-class structure. Animals are at least and probably more sensitive to age or forest stage than to forest type and maybe even area. Proportion of forest in each age class is a major question, the answer is the age-type-acre to which animals respond. I presume all is done based on size classes (appendix 6) but the relations should be made more explicit. I have to assume (?) "saw" size includes age 40 and 50 throughout.
- p.170 Note: Not only species but age constitutes the food-producing (or other benefit) tree.
- p.172 mid page - "Rotation length", often used, can be a leading phrase. Variable scheduling of harvest periods and tract sizes to achieve explicit conditions for trees, animals, soil erosion status, etc. can be developed (see my 1978 Wildlife Management textbook for suggestions). I suggest you use "ecologically-based harvest schedules" or some other related phrase. Area-regulation or volume-regulation strategies will not optimize for a set of wildlife benefits.
- p.177 Add: placement of large portions of slash in streams to restore stairstep features of the former forested riparian system.
- p.184 6.13 These don't hurt, but be careful. At 5-6 pounds per day per 100 lb. deer, how many deer days can such areas in Minnesota supply? within a group (herd's) range? These may be the core areas, very fertile, intensively managed at high cost and thus at least one forest forage substitute.
- p.184-5 Consider means of increasing the value of animals, reducing losses (law enforcement), and increasing better use of those present.
- p.184-5 Consider a deer increase then its impact on the young forest. How will this effect be reduced?
- p.188 Consider adding: Thinning around superior canopy mast producers can increase mast production.

- p.193 line 2 - caps
- p.193 6.11 - Proven impacts are secondary. They have been used to increase browse. This paragraph seems unnecessarily slanted.
- p.193 6.12 - education, signs, etc. seem worth noting.
- p.194 Ample studies show the net effects of forest and wildlife management together can exceed efforts to maximize forest profits alone (e.g. Forestry at max = 10 units; forestry (8) with wildlife (8) = 16 units of return). Tally tourism, sales, services; tally nutrients; tally improved site index due to soil texture improvement, fungal enhancement of root uptake of nutrients, and pest insect control.
- p.195 line 8 - Insert "and diverse returns from the extensive wildlife resources of the State."
- p.195 last line - Critical loss or change. Note that allowing a stand to age excessively may pass it out of the "service" of a particular species or group.
- 2-8 2 lines from bottom - but wood duck having increased, available habitat under more intensive use seems very unusual - shorter rotations, reduced area and size, etc.? Last line: relation of aging (time) to riparian forests (place) seems unclear; it is a big assumption - no riparian harvests. This 200 foot zone and "no harvest" seems one of the most difficult assumptions. I recommend extra commentary about it.

See James Fraser's Ph.D. on Minnesota eagles and human influence.

Raccoon changes should be consistent with riparian assumptions above. Crayfish etc. of the stream environment plus large stable trees suggests increases not decreases as reported here in the appendix.

Red-headed woodpecker line 15 replacing cavities? (words missing?)

Loggerhead Shrike - problematic analysis - Since not forest related and rare in recently clear cut land, it is hard to understand why increased habitat (rarely if ever used) is even stated. The last two sentences seem correct; the projection is misleading.

Chat need not be emphasized as riparian, only open clearcuts, early succession forests, old fields.

Cardinal - High use of feeders is true, but the magnitude of the effect on the populations of the entire state must be questioned. This need not be pushed; effects of harvest +/-

related to area over the long run certainly far surpass feeder effects. I recommend keeping it a simple observation.

Lincoln's Sparrow - Observations on a model that grows trees and moves them from stage to stage at different rates within each ecoregion seems at odds with what I read previously about the procedures used. I will re-read, but there seems to be a mis-match or low clarity between what is suggested in last paragraph and earlier text. Please check; I shall also.

Orchard Oriole - As with a few others, the approach used may not model the population well (3 lines from bottom). Perhaps a summary paragraph listing such species and suggesting uncertainty and need for further work will reduce future conflict.

The Following Details the Responses to the Review
Forwarded by Robert Giles

The reviewer stated that overall the report was excellent, and did not give any major general comments. The reviewer provided a number of helpful editorial comments that pointed out missing words or apparent inconsistencies in numbers cited in various parts of the text. All were checked and corrected where necessary. A response to a few more major points follows:

Page 77: The reviewer comments that small rodents should increase in the long run with more harvest, leading also to an increase in the timber rattlesnake, and questions how the changes are accounted for in the periods after the first decade. The authors point out that calculations were made separately for each decade, given the forest age distribution at each decade, so that the 50-year effects of harvest in the first decade is taken into account. The timber rattlesnake is listed as of special concern in Minnesota, because the state is at the extreme edge of its range. In Minnesota's climate, populations may not respond as in other areas because climate is a limiting factor in addition to the number of prey.

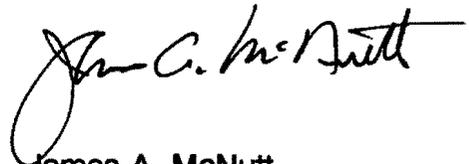
Changes made to the document: None

Page 167: The reviewer expressed concern that most animal species are sensitive to forest age class. This is taken into account in all of the analyses, as explained in section 3.

Changes made to the document: None

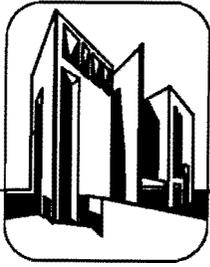
After review by the Advisory Committee, please advise me if there are any significant issues you regard as outstanding. In the absence of such, this paper will be submitted to the EQB for formal approval.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager

November 25, 1992



University of Wisconsin-Green Bay
2420 Nicolet Drive, Green Bay, Wisconsin 54311-7001

21 November 1992

Dr. James A. McNutt
Executive Vice President and
GEIS Project Manager
5510 Six Forks Road
Raleigh, NC 27609

Dear Dr. McNutt:

Enclosed is my formal review of the technical paper entitled *Forest Wildlife*, a work product completed in conjunction with the Minnesota Generic Environmental Impact Study.

My submission of this review does not place me under any obligation, legal or otherwise, to the State of Minnesota or Jaakko Poyry, Inc. on matters pertaining to the GEIS work and end product.

Furthermore, I recognize that this review will become part of the public record of the State of Minnesota as related to the GEIS project.

Respectfully Yours,

A handwritten signature in black ink, appearing to read "Robert W. Howe".

Robert W. Howe, Ph.D.
Associate Professor
Department of Natural and Applied Sciences

cc: Ek, Kilgore

MINNESOTA GENERIC ENVIRONMENTAL IMPACT STUDY
 TECHNICAL PAPER REVIEW SYNOPSIS FORM

Forest Wildlife
 Technical Paper Title

Reviewer details:

Name: Robert Howe

Date Received: _____

Title/Position: Associate Professor

Due Date: _____

Address: Dept. Natural and Applied Sciences
Univ. of Wisconsin-Green Bay
Green Bay, WI 54311-7001

Paper Component

Rating (circle one)

Literature review	1	②	3	4	5	6
Evaluation of Existing Environment/Conditions	1	②	3	4	5	6
Significant Impacts - Assessment Methodology	1	2	③	4	5	6
Significant Impacts - Assessment Results	1	2	③	4	5	6
Mitigation Measures - Appropriateness	1	2	③	4	5	6
Mitigation Measures - Utility	1	②	3	4	5	6
Overall Technical Quality	①	2	3	4	5	6

Rating System

Adequate, no changes needed	1	
Some concern(s), no changes needed	2	(see attachment)
Moderate concern(s), some changes needed	3	(see attachment)
Major concerns, significant changes needed	4	(see attachment)
No comments	5	
Comments not required	6	

Reviewed by: Robert W. Howe

Date: 11-21-92

**Minnesota Generic Environmental Impact Study
Technical Paper Review Comments**

Forest Wildlife

Reviewer: Robert W. Howe, University of Wisconsin-Green Bay

The general approach used by this technical paper to predict the consequences of three harvest levels is reasonable but hardly precise, largely because information about habitat relationships is quite incomplete. Criticism aimed at incomplete data is an empty one, however, because little more than qualitative arguments are available as a substitute. I will try to aim my comments toward points that can be changed constructively in the document, admitting from the start that the authors have already presented a sound analysis given the information available.

I have little criticism of the data presented on species' habitat preferences: one can hardly argue with direct field observations by the authors and their colleagues, many of whom are among Minnesota's most knowledgeable naturalists. Nevertheless, I believe that the numerical projections of response (e.g., Fig. 4, Table 7) really need to be more carefully qualified. Again, I understand that the GEIS format demands quantitative predictions, and I applaud the authors' clarity in describing their methods. But let's face it, these projections are often little more than educated guesses. Perhaps most seriously, they rely on several assumptions that are not likely to be valid:

1) *Climate and other external circumstances will not change significantly during the next 50 years.*

Even ignoring the potential for global climatic change, one can be fairly sure that events in the wintering grounds of long-distance migrant birds will change the mortality rates of certain species (for some species positively, for others negatively). Surely these uncertainties can't be built into the numerical projections, but they are just cause for taking the projections less seriously and, most importantly, for accepting the mitigation measure described below.

2) *Harvest patterns will not significantly affect the existing configuration and juxtaposition of forest tracts.*

Although the discussion identifies edge effects and the sensitivity of certain species to habitat area, these considerations are not built into the population projections. Again, I'm not contending that they ought to be (we are even more ignorant of these issues), but the assumption ought to be identified up front.

3) *Species interactions will not be altered as a result of the projected harvest schedules.*

In my opinion this is the most serious shortcoming of the modeling approach used in the GEIS. Once more, consideration is given in the text (e.g., the effects of brainworm interactions between deer and moose), but the population projections ignore such effects entirely. If experience serves us correctly, an increase in timber harvest and related disturbances will lead to increases in certain ecologically significant species (e.g., Brown-headed Cowbird, American Crow, Masked Shrew (?), Great Horned Owl (?)) which are likely to have a negative impact on other species above and beyond the parallel changes in habitats. We seem to find in eastern Wisconsin that House Wrens, for example, have an impact on Winter Wrens in areas that have reduced forest cover (Wolf and Howe 1990, *Passenger Pigeon* 52(2)). Reduction of Black Bear numbers might lead to higher White-tailed Deer populations due to lower fawn mortality. Woodland Jumping Mice might

be affected seriously by the increase in Meadow Jumping Mice. The list of plausible species interactions that will be affected is long but, admittedly, speculative. The fact that our ignorance is great doesn't mean that these interactions will be unimportant, however, and a stronger admission of the uncertainties that they engender is warranted.

4) *Abundance of a species in a habitat category is an adequate indicator of population productivity.*

The concepts of source-sink population dynamics are well enough established today (Pulliam 1988, Howe, Davis and Mosca 1991, Gilpin and Hanski 1991) that they ought to at least be considered in an analysis such as this. Empirical data are much more difficult to find, but our field work in Wisconsin (and I'm sure experiences of many others, if viewed from a metapopulation context) show that presence of a species is not always a good indicator of demographic productivity. In the Nicolet National Forest during 1991 and 1992, Veeries were present in intensively studied hardwood forests, but breeding was poor to nonexistent. Frankly, the abundances shown in Table 5 are generally consistent with our findings in Wisconsin, but I'm still uncomfortable assuming that replacing x hectares of "sawtimber" hardwoods with x hectares of "pole sized" aspens will have no demographic effects on Black-throated Green Warblers, Black-and-white Warblers, Scarlet Tanagers, American Redstarts (which are all but absent in aspen stands of northeastern Wisconsin), Yellow-throated Vireos, and other species.

5) *All sites of a given habitat category within a given ecoregion are qualitatively identical.*

Clearly, a tract of "upland pine" forest along a stream is far more important to a Northern Waterthrush than a tract of "upland pine" far from water, yet the model doesn't appear to make any such distinction. Several species (e.g., Magnolia Warbler) seem to have much more specific habitat needs than are captured by the rough categories used in the GEIS. Likewise, many species of birds (and I suspect small mammals) are quite localized in their distributions, yet the closes resolution available is the "Ecoregion." I don't have a good solution to this problem, but I think the intellectually honest approach is to identify this problem up front.

Other assumptions of the model should be articulated more clearly in a more prominent place (p. 52). Consideration of these assumptions, I think, confirms my earlier point that the population projections harbor a great deal of uncertainty.

What should be done to mitigate the proposed harvest levels? I think the most important measure would be to *mandate a comprehensive, long-term monitoring program* funded by a tax on timber harvest receipts. If our information is inadequate today, it will be inadequate tomorrow unless new information-gathering initiatives are implemented. The most sensible approach to any large scale program of biological harvesting is to develop and maintain a program of "adaptive management," whereby affected wildlife populations are monitored carefully while the harvest plan is being implemented. If populations of desirable species decline, then the plan should be modified to avoid serious consequences. The burden of costs should fall on the beneficiaries of the harvest, not on the general tax revenues or, worse yet, the dedicated efforts of volunteers. Just as economists rely on quantitative indicators to describe current conditions, forest ecologists and wildlife biologists should rely on ongoing field data to assess the impacts of timber harvest prescriptions. This will be an effective approach if decisions today are made with the recognition that plans might need to be modified as the harvest schedules are implemented.

The remainder of my review consists of notes regarding specific points or portions of the text. I present them in the order which I encountered them in the text.

- p. 6-7. The discussion of mammals is excellent, though I suspect that species interactions among small mammals are more important than the author acknowledges. I am not convinced that important interactions between *Peromyscus leucopus* and *P. maniculatus gracilis*, *Sorex cinereus* and *Sorex hoyi*, and the two jumping mice are not affected by forest management practices.
- p. 31. The text correctly states the importance of interactions between Red-tailed and Red-shouldered Hawks (and could have mentioned similar interactions between Great Horned Owls and other species) and even goes so far as to say that these factors "need to be considered", but I fail to see where and how they are considered later in the analysis. Maybe I just missed it.
- p. 34. Citations for Helle (1986), Wilcove and Robinson (1990), as well as others are missing.
- p. 47-48. Section on Habitat and Wildlife Population Linkages is well done.
- p. 52. The *Assumptions* section needs to be expanded, as argued above.
- pp. 71-75. This section responds to some of my objections, as you no doubt have noted. Some recognition of these and other important limitations needs to be mentioned in the *Summary* (page i), which will be the only part read by many people interested in this report.
- p. 112. Given the widespread increase in White-tailed Deer in the Upper Midwest during historic times, I find the projected declines to be surprising (though I have no evidence to refute it.)
- p. 125. Competition for cavities between Northern Flicker and other cavity nesters ought to be considered.
- p. 181. The mitigation strategy for riparian zones is extremely important, not only for riparian species themselves but also to maintain a corridor system that (arguably) will be important to maintain the integrity of forest metapopulations.
- p. 171-195. The other mitigation alternatives clearly will benefit wildlife and are well articulated. If they could be more specific (e.g., how many large blocks of uncut forest should be set aside?) they will be more effective, I think.
- the literature review ignores some significant and relevant work in nearby states, notably a series of articles in Wisconsin's *Passenger Pigeon* by Hoffman and Mossman.

The Following Details the Responses to the Review
Forwarded by Robert Howe

The reviewer states five assumptions which are implicit in the analyses. All five are valid points, but they are aimed more at the MN GEIS process, and the questions the authors were asked (by the Final Scoping Document) to answer than to the analyses themselves. The reviewer acknowledges in the introductory paragraphs that the analyses are sound given the information available.

Assumption 1:

Climate and other external circumstances will not change significantly during the next 50 years. The authors were not asked to take this into account, only to determine the current amount of wildlife habitat and amount that will be gained or lost due to timber harvesting and forest management only.

Changes made to the document: A statement was added that this is not part of the scope of the document in the beginning of section 4.

Assumption 2:

Harvest patterns will not significantly affect the existing configuration and juxtaposition of forest tracts. The authors have already acknowledged the lack of data in this area in sections 3.4.1, point 3 and section 3.4.2, sections 2 and 3.

Changes made to the document: None

Assumption 3:

Species interaction will not be altered as a result of the projected harvest schedules. The reviewer points out that he thinks this is the most important flaw overall. However, again, the authors were asked by the Final Scoping Document and required by significance criterion number 11 to examine the amount of habitat available under differing harvest scenarios. The authors agree that species interactions are important, and this is mentioned in several places even though not required. Section 2.2, moose-deer interaction and section 2.3 forest interior birds, have extensive discussions of species interactions.

Changes made to the document: This is now listed with the assumptions in section 3.3.1.

Assumption 4:

Abundance of a species in a habitat category is an adequate indicator of population productivity. Again, this is outside the scope of the document. Significance criterion number 11 requires analysis of amount of habitat.

Changes made to the document: None

Assumption 5:

All sites of a given habitat category within a given ecoregion are qualitatively identical. This relates to assumption number 2, where the authors point out locations in the document where this shortcoming is already acknowledged. In some cases, however, these factors were taken into account. For example, in the analyses of white-tailed deer, there had to be some young aspen and some pole or mature conifer in each township in northern Minnesota for that township to count as good deer habitat. In addition, not all "upland pine" or any other similar category was rated the same for many of the bird and small mammal analyses. Small mammals had weights for each cover type and size class, and some of the bird analyses selected FIA plots within a certain distance of water to construct habitat matrices. Although taking this type of spatial data into account is very difficult, it was done to the extent possible by the wildlife study group.

Changes made to the document: None

General:

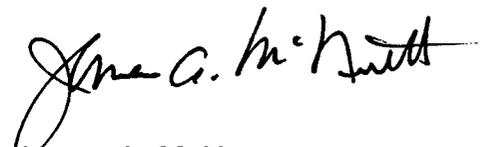
The reviewer also makes the point that monitoring of wildlife populations and adapting management when there appears to be a negative change would make a good mitigation. The authors agree, however, this is a general statement with regard to mitigations rather than a mitigation strategy itself.

Changes made in the document: Monitoring has been acknowledged in the beginning section of the mitigation chapter.

The remainder of the review consists of specific points for minor improvements in the text. All were considered and several were implemented in the document.

After review by the Advisory Committee, please advise me if there are any significant issues you regard as outstanding. In the absence of such, this paper will be submitted to the EQB for formal approval.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager

November 25, 1992

JANET C. GREEN
1754 Old North Shore Rd.
Duluth, Minnesota 55804

DATE: December 19, 1992

TO: Mike Kilgore, EQB project manager for the GEIS on Timber Harvesting

FROM: Janet Green, Member, EQB Advisory Committee, GEIS on Timber Harvesting
(Minnesota Audubon, Forestry/Biodiversity Program Coordinator)

Final comments on the FOREST WILDLIFE technical paper

Preferred Mitigations:

The constraints that were modelled in the second run need to be explicitly identified as mitigations (i. e. extended forest rotations, uneven-aged management, old growth reserves, BMP's in riparian zones, wildlife buffers in riparian zones) to make the analysis in this document valid. These constraints are NOT policy now and need to be part of an explicit mitigation strategy.

Technical points:

p. 151. Last sentence should read "confound projections of [positive] and negative trends for many species". Species that are projected to have a positive increase might be negatively effected by increasing populations of cowbirds.

p. 153. (cell 2) in the upper right

Note: It would be very useful to have a diagram similar to Figure 4.49 for the medium harvest scenario. It is there that the policy implications are most important and this kind of diagram provides a good summary of vulnerability of specific species, not just numbers of species. It is the particular species that is important for conservation strategies. See p. 158.

p. 156. Increasing vulnerability going downward.

p. 165. "BMP's . . . do not . . . address protection of small wetlands". How does this statement relate to the way this issue was covered in the Water Quality and Fisheries paper?

p. 172-73. This list should be in taxonomic order to make any sense.

p. 188. The guidelines for cavity dependent birds need to be described before any statement about "scale" being appropriate is made. What does "scale" mean in this context? What are the guidelines and what habitat requirements do they address? Guidelines for eagles and colonial birds are explicitly described. The same should be done for cavities and snags. What evidence is there that these guidelines are actually practiced in the field?

The Following Details the Responses to the Comments on the Wildlife Technical Paper Forwarded by Advisory Committee Member Jan Green

Preferred mitigations. The reviewer states that a number of mitigation strategies in the second runs of the forest scheduling model are not policy and need to be part of an explicit mitigation strategy, including: ERF (extended rotation forests), uneven-aged management, old growth reserves, BMP's in riparian zones and wildlife buffers in riparian zones. However, all of these are addressed as mitigations in the Wildlife paper (except old growth, which is handled in the biodiversity paper). The wildlife study group decided to list these as mitigations, even though they were to some extent incorporated in the model runs, to make the point that they do need to be considered for future policy, and that the extent of the mitigations incorporated in the model runs was not sufficient to mitigate all impacts. See mitigations 6.2 (p. 184), 6.3 (p. 185), 6.9 (p. 193).

Technical Points.

P. 151. The reviewer wants the sentence changed to read "confounding of both positive and negative trends".

This change has been made.

P. 153. A typo on the fourth line from the bottom was detected by the reviewer. "(cell 2)" is correct, not "(cell 1)".

This change has been made.

P. 156. The reviewer points out that the arrow on Fig. 4.49 should point up. In fact, the words "decreasing vulnerability" should be along the y-axis, accompanied by an upward pointing arrow.

This change has been made.

P. 165. The reviewer quotes from the paper that "BMPs ... do not.. address protection of small wetlands", and asks how this relates to the way that this issue was covered in the water quality paper? The study group does not understand this question, but again points out that protection of small wetlands (page 193, bottom), is identified as a mitigation strategy for protection of herps.

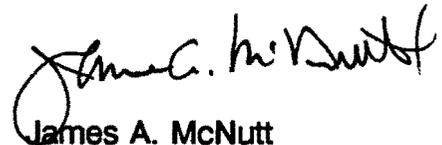
P. 172-173. It was suggested the list be in taxonomic order.

The table has been reordered to reflect this order.

P. 188, mitigation 6.5. The reviewer would like explicit guidelines stated on page 188. However, as pointed out in the narrative on page 188, we don't really know exactly what the guidelines should be for most species, which have not been studied as much as eagles. Therefore, the wildlife study group stated that the MNDNR and Forest Service guidelines should be a minimum. The study group should probably have stated at the top of the mitigation that research needs to be done to determine guidelines for most species, and that the MNDNR and Forest Service guidelines should be used in the meantime, until we know more. This is stated in the narrative that accompanies the mitigation language.

This review was used as noted to revise the technical paper, as was appropriate, along with the other written Advisory Committee Members' review inputs. Additionally, the paper received one more careful editorial review for spelling punctuation, and basic presentation style, and all final edits have been incorporated into the text. This response is therefore submitted to the EQB to become part of the formal public participation review package for the GEIS process.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager

December, 1992

Forest Wildlife

Gerald A. Rose • DNR • Comments on GEIS Technical Papers

Overall, this draft of the Wildlife paper is very well done, and much improved from the first draft. The authors should be complimented on a very good product. It also appears that DNR's comments on the original draft were considered by the team, and most were incorporated in the analysis and report. The methodology used to assess impacts is generally sound, and the results seem credible and logical. The discussion of mitigation strategies was also improved over the earlier draft, and the analysis of preferred strategies appears reasonable. The following are some comments provided to strengthen the document and ensure that the paper is accurate and complete:

Executive Summary

Of all the technical reports, the greatest public interest will be in the wildlife report. Nevertheless, many will probably read little more than the executive summary. As presently written, this summary does not fully represent the content of the report. The authors also did not address a key recommendation for revision that the DNR made in the first draft. At a minimum, we suggest that the summary include at least two or three paragraphs that provide an overview of Section 3.4 - "Limitations" and some of the assumptions used in the analysis (see below), and acknowledge some of the basic problems associated with the analysis beyond those mentioned on page ii.

Assumptions

There are a number of assumptions that underlie the report and potentially influence the impacts assessed for many species. These include the assumptions that: 1) selective cutting in riparian habitats has no impacts on species associated with that habitat type; 2) 40 acre stand sizes are sufficient for forest interior bird species; 3) increases in populations of brood parasites such as brown-headed cowbirds and predators such as blue jays, grackles, great horned owls, striped skunks, and raccoons will have no effect on other species; 4) there will be no change in the distribution and size of forest patches of various forest types and age classes; 5) extended rotation forests (ERFs) will provide the same habitat features as unmanaged forests; and 6) extended rotation forestry is in effect on 20% of the state and federal timberland. (Currently, ERF is practiced only by Chippewa National Forest and only on 10% of their land. ERF is likely to become policy on state land in the relatively near future, but implementation of ERF on the Superior National Forest is uncertain).

The significant impacts predicted for the species evaluated at the three harvest levels should be regarded as reasonable estimates of how these species will fare in the future, provided these and other assumptions are met. Since many of these assumptions are not likely to be met, it is important to note that the nature of these assumptions, both

those in the timber harvest model and those that were necessary to model future population trends, suggests that the impacts to the wildlife populations predicted in this paper may be underestimates of real long-term effects of timber harvest on populations of many species. Consequently, these predictions should be accepted by the EQB with the following caveats:

- 1) These predictions are the best scientific estimate available at this time.
- 2) Monitoring of populations should be done to allow appropriate management responses.

Species Groups

The authors of all four sections took on an extremely difficult task and produced a very satisfactory report. The discussion of bird species vulnerability (Section 4.3.4) was especially innovative, but could have been useful for the other species groups, particularly herps, which suffered from a lack of quantitative data. The analysis of large mammals was greatly improved from the first draft, and the incorporation of a spatial analysis to assess availability of winter cover and browse for moose and deer strengthened this analysis. However, the importance of conifer winter cover may have been overemphasized, and resulting impacts probably should be considered worst-case.

As Bob Howe indicated in his review of the paper, the report would have benefitted from additional focus on species interactions. While we agree that such effects would be quite difficult to model, more emphasis could have been given to those species likely to be negatively or positively affected by predicted increases or decreases in other species (i.e., due to predator-prey interactions or effects of brood parasitism and predation).

We are concerned that the Forest Wildlife paper may not adequately address impacts of timber harvesting on some types of wildlife associated with wetlands (especially reptiles and amphibians). This issue is discussed on pages 3-4 of this memo.

Mitigation Strategies

The mitigation strategies are in some cases too vague (e.g., Section 6.5, Retention of More Trees With Cavities, p. 188) or inappropriately specific (Section 6.11, Reduce Use of Herbicides, p. 194-195). This inappropriate level of specificity may become a problem as the draft GEIS is developed, and as public agencies attempt to implement the strategies once the GEIS is completed.

The mitigations presented in the Wildlife Paper are for the most part both feasible and likely to be effective. Just how effective they will be is unknown. There is a pressing need to develop and maintain an adaptive management program as suggested

by Bob Howe in his review of this paper and by Tom Crow in his review of the Biodiversity paper. The authors acknowledge the need to develop a comprehensive and uniform data base on wildlife habitats and populations in the last two paragraphs of the text (p. 208), but this key recommendation seems like an afterthought. In their response to Dr. Howe's peer review, however, the authors stated that population monitoring is not a mitigation strategy (November 25 letter from J. McNutt to M. Kilgore).

Inventory is a primary mitigation in the Biodiversity Paper, and should be considered essential for the Wildlife paper to safeguard against depleting important wildlife habitats and populations. In fact, it can be argued that this is a more important strategy than any of the others listed in the paper. Therefore, **we strongly recommend that a mitigation strategy be added that would explicitly recommend comprehensive, long-term monitoring of wildlife populations, and a procedure to modify management as a result of this monitoring.** Modified management should incorporate other preferred mitigations as well.

The no-net-loss mitigation referenced on page 207 and on page iv of the summary has limited value unless it is applied on an ecoregion basis. From a wildlife standpoint, it would be

unacceptable to mitigate losses of forest lands in one ecoregion with gains in forests in other regions. These paragraphs should be clarified in the text.

The Following Details the Responses to the Comments on the Wildlife Technical Paper Forwarded by Advisory Committee Member Gerald Rose

Executive Summary.

The study group points out that some major limitations are already given in the first paragraph on page iii and in the last paragraph of the summary. However, a new sentence has been added at the end of the first paragraph on page ii that briefly highlights some of the other major limitation of the analysis of wildlife populations.

Assumptions. The study group agrees that the reviewer's list of assumptions may not all be reasonable for all species. Some of the assumptions were forced both by lack of critical information in the FIA data to adequately assess these assumptions and by the fact that there is little or no information regarding the relationship between Minnesota's forest wildlife and these assumptions. The overall impact of incorporating these assumptions in projections results in a conservative, or underestimation of harvesting impacts. For example, the 40 acre stand size for interior bird species was selected based on data from the northeast and central United States and by examining the distribution of stand area in southern Minnesota. For many interior bird species, this is a minimum estimate of the patch size required to sustain a breeding population. If a larger minimum patch size had been selected, then there would be fewer suitable patches, and the projected negative impact of harvesting would have been greater for many species.

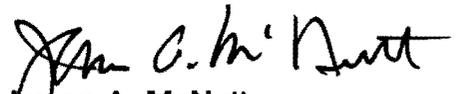
Species Groups. The reviewer states that the importance of conifer cover for deer may have been overemphasized, and that projected impacts should be considered worst case. No significant impact was identified in the forest wildlife paper for deer. Why is the reviewer concerned that the impacts are "worst case"?

Mitigation strategies. It was suggested a mitigation strategy be added regarding monitoring. However, monitoring is not a mitigation strategy. It is how the effectiveness of mitigation (or lack of mitigation) is evaluated over time. This is the type of issue that should be discussed in the GEIS. The study group does support the need for monitoring over the next 50 years.

The reviewer states that no net loss of forests should apply to each ecoregion, so that loss in one part of the state cannot be "compensated" for by gains in another part. The study group agrees, and both the summary and page 204 have had the words "by ecoregion" added to the no net loss of forest area statements.

This review was used as noted to revise the technical paper, as was appropriate, along with the other written Advisory Committee Members' review inputs. Additionally, the paper received one more careful editorial review for spelling punctuation, and basic presentation style, and all final edits have been incorporated into the text. This response is therefore submitted to the EQB to become part of the formal public participation review package for the GEIS process.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager
December, 1992



Minnesota Forest Industries, Inc.

1015 Torrey Building
314 West Superior Street
Duluth, Minnesota 55802

Phone (218) 722-5013
Fax (218) 722-2065

December 23, 1992

**Dr. Michael Kilgore
GEIS Project Manager
300 Centennial Building
658 Cedar Street
St. Paul, MN 55155**

Dear Dr. Kilgore:

Please find attached my comments on the Forest Wildlife Technical Paper that has been developed as a resource for preparing the Draft and Final GEIS. It is my understanding that these comments will be included as part of the final published technical paper. I greatly acknowledge the help of Dr. T. Bently Wigley, Forest Wildlife Scientist, Department of Aquaculture, Fisheries and Wildlife, Clemson University, in preparing these comments.

Sincerely,

Wayne Brandt



Minnesota Forest Industries, Inc.

1015 Torrey Building
314 West Superior Street
Duluth, Minnesota 55802

Phone (218) 722-5013
Fax (218) 722-2065

FOREST WILDLIFE TECHNICAL PAPER

The primary data set utilized for modelling to prepare the GEIS technical papers is the Forest Inventory Analysis (FIA). Using this data as a predictor for habitat quality and quantity and then further as a surrogate for predicting wildlife populations over the 50 year period of the GEIS has inherent limitations.

These limitations are important as this type of modelling has not previously been done in Minnesota. No effort has been made to rectify or quantify these limitations and their influence on the model results. Since this was not done, additional efforts should have been undertaken throughout the paper to further describe the effects of the data, limitations, the uncertainty in analysis that they cause and their influence on the papers analysis and conclusions.

It must also be noted that the modelling used for the Forest Wildlife Technical Paper remains untested. Extrapolating habitat quality and population predictions from FIA data is not something that is routinely done. While not required by the Environmental Quality Board, it would have been useful to field test the model. Validation of the model and its predictive quality could have been attempted by using historical FIA data from 1936 and projecting forward based on known harvesting activity to predict present levels of some wildlife populations and their habitats. This could have added some certainty to the predictions made and the methodology used.

Also inherent in the methodology of the GEIS was the need to decide which species for the various taxonomic groups would be analyzed. While this was necessary, the choices made are likely to have had some impact on the outcomes of the analysis.

Further data problems include the limited knowledge that exists about wildlife populations and wildlife/habitat relationships. Much of the available data relates to habitat use as opposed to habitat requirements or needs. The paper does, in a brief manner, describe some of these problems but then moves on without extensively revisiting this issue.

This data problem is particularly acute, as acknowledged in the paper for the section on amphibians and reptiles. It is also a problem for birds. The paper states that the data for 75% of the birds analyzed was based on the work of two individuals for a recent two year period of time.

Clearly, in order to prevent the introduction of bias or methodological anomalies, larger and broader data sets should have been utilized. In the alternative, the predictions should have been further qualified.

The paper describes the methodology used where data was absent for birds (25% of the bird species utilized). This methodology, which involved a workshop/seminar discussion seems unscientific given the conclusions that are drawn and the manner in which they are described.

One of the areas of particular controversy in the Forest Wildlife and Biodiversity Technical Papers has been the analysis of conifer understory in regenerated forests. While the biodiversity paper continues its inherent bias in not accepting data which shows the continuation of conifers in similar percentages for like age classes in regenerated stands, it is not clear how this has been dealt with in the wildlife paper. Previous drafts of the wildlife paper exposed this error but this final draft does not seem to address the issue.

In the sections on mammals the "populations index" should be clarified and the magnitude of difference among categories standardized among taxonomic groups of wildlife.

The authors should clarify what constitutes "low", "moderate," and "high" abundance of mammals. Why is a habitat with a "high" abundance of mammal twice as valuable as one with a "moderate" abundance? Why is a "high" abundance habitat 5 times as important as one with a "low" abundance?

The abundance scales should probably have been standardized among taxonomic groups and justified biologically. The abundance categories for mammals (page 57) are 0 (absent), 2 (low), 5 (moderate), and 10 (high). With birds, however, the values are 0,1,2,3,4, and 5, for the categories of absent, very rare, rare, uncommon, common, and abundant, respectively (page 68). Why were different scales used?

In the sensitivity tests that were conducted for the mammal scale (described on page 57), there was very little variation in the magnitude of difference between categories. High-abundance habitats were 2 or 3 times as important as moderate-abundance habitats which in turn were 2 or 3 times as important as low-abundance habitats. The model is likely sensitive to the difference in magnitude among categories. Thus, the authors should probably have run other sensitivity tests for mammals using a scale similar to birds (with differences among categories of 1).

The application of the significance criterion for this issue is inconsistent. Significance criterion number 11 states "An impact is considered significant if the available habitat of a species is projected to be changed by 25 percent in any ecoregion". This EQB approved criterion was modified prior to approval to reflect change in habitat as opposed to only analyzing decreases in habitat.

The application of this criterion throughout section 5 only relates to decreases in habitat. This clearly contravenes the approved language of criterion number 11. The contractor argued during the GEIS Advisory Committee meeting that this was appropriate because the point of applying the criterion was to develop mitigation and that positive impacts would not be mitigated.

The purpose of applying the criterion is to determine the impacts that meet the test of significance based on the approved criterion. The issue of mitigation is appropriately addressed in the mitigation section of the paper (section 6).

Finally, the effectiveness of some mitigative measures is unverified. The benefit of travel corridors for many species has yet to be demonstrated through research. Almost all of the literature promoting their use are "opinion" papers. For some species their value is doubtful. For example, why should neotropical migratory birds, which travel thousands of miles need corridors to colonize forests? Likewise data to support the requirement of 200 foot buffers on streams and lakes is lacking.

The Following Details the Responses to the Comments on the Wildlife Technical Paper Forwarded by Advisory Committee Member Wayne Brandt

Modelling population index (The reviewer's first and second pages). The reviewer states that population indexes and habitat abundance values may be a source of error in the projected populations of wildlife species. Different methods are used to sample different taxonomic groups (birds and mammals). The ability to discriminate among different population levels differs with taxonomic group, and there are likely inherent differences in spatial variability for different groups. Therefore, there is not an obvious, a priori reason that the same scales and increments are desirable for different groups.

The approaches used to assess possible impacts of modelled timber harvest on small mammals are certainly sensitive to the difference in magnitude, as the reviewer points out. However, the sensitivity tests used to examine this issue showed that the differences were relatively minor among the several possible scales considered for use in the habitat and population index analyses. All of these scales are almost certainly conservative, and probably underestimate real-world differences in abundance between extremely poor habitats and the best habitats. Use of any of these scales (including that finally chosen) in the analysis probably tended to underestimate the magnitude of modelled impacts.

Another point is that the greatest overall uncertainty in analysis of wildlife populations lies in the modelling of forest harvesting and FIA data provided the Wildlife Study Group by the Maintaining Productivity and the Forest Resource Base study group. This uncertainty greatly outweighs the uncertainty in the relative abundance values assigned to bird and mammal species. See the example on page 69 (table 3.6). The change in habitat area predicted by the forest change and scheduling model is the biggest factor in the projected change of the species population. There are areas of low, medium, and high abundance both before and after the 50-year planning period. It is the forest change model that predicts the change in acreage of each habitat type.

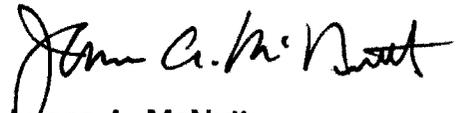
Positive significant changes. Bottom paragraph on page 2, and top paragraph on the reviewer's page 3 simply states his opinion for the record that both positive and negative changes can be significant. The study group notes that all of the data necessary to interpret positive changes for all species is present in the results section (4 Projected Impacts) of the Wildlife Paper. Positive impacts are also recapitulated briefly in the Summary.

Conifers in mixed stands. In the third paragraph on the third page, the reviewer mentions the conifer issue again. See the response to Mr. Jim Woehrle's review on the biodiversity paper.

Mitigation effectiveness. The reviewer ends his comments by stating that the effectiveness of some mitigations is unverified. This reiterates the cautions about mitigations put forth by the study group on pages 183-184.

This review was used as noted to revise the technical paper, as was appropriate, along with the other written Advisory Committee Members' review inputs. Additionally, the paper received one more careful editorial review for spelling punctuation, and basic presentation style, and all final edits have been incorporated into the text. This response is therefore submitted to the EQB to become part of the formal public participation review package for the GEIS process.

Yours sincerely,



James A. McNutt
Executive Vice President
and GEIS Project Manager

December, 1992