

An Ecosystem Spatial Analysis for Haida Gwaii, Central Coast and North Coast British Columbia

Review by

J. P. (Hamish) Kimmins
Professor of Forest Ecology,
Director, Forest Ecosystem Management Simulation Laboratory,
Senior Canada Chair in Ecosystem Modeling
Department of Forest Sciences
Faculty of Forestry,
UBC
Vancouver

**Member, UNESCO World Commission on the Ethics of Scientific Knowledge
and Technology**

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Executive Summary

- Much of my review focuses on the terrestrial sections of the report, although I read the report in its entirety, since this study was commissioned in support of an LRMP process that focuses largely on the forested and non-forested land and associated freshwater systems. My expertise and experience is mainly in the ecology of terrestrial ecosystems, especially forests, although I have some experience in freshwater systems. The important linkages between the terrestrial /freshwater systems and the marine system receive little attention in this lengthy report
- I found that, within the limitations of my knowledge, the freshwater and marine sections of the report appear to be more rigorous and credible than the terrestrial section, which covered topics I am more familiar with. In part this may be because these two sections appear to have used more appropriate spatial scales in their analyses, and to be more ecosystem-based, whereas the terrestrial section used inappropriate temporal and spatial scales, and seems to be biased towards medium to large vertebrates; a vertebrate conservation biology focus rather than the conservation ecology focus (that includes conservation biology) that I believe was needed. My evaluation may also reflect differences in the depth of my knowledge and experience between these three fields of science.

- The goal of modeling terrestrial conservation issues for the Central and North Coast and Haida Gwai within the time, data and human resources available (or used) was unreasonable and overly ambitious. To attempt to do this and produce science-based conservation recommendations from the modeling was politically inevitable, but was unwise considering the data inadequacies and the tools that were selected for the analysis. The focal species models may be the best available, but many failed to perform adequately - a reflection of the inadequacies of the data base for building these models. This prevented mechanistic, “causal”, explanatory predictive modeling and led to the use of generally very simple, correlative models. The integrating model, SITES, is judged to have been unsuitable because of its failure to address appropriate spatial and temporal scales. The report is a useful statement of what we do and do not know about the focal species in the study area, and of the problems with the models that were used. As a result of these modeling problems, the authors’ conclusions about conservation area requirements, at least for the terrestrial section, were based on a mixture of philosophy, opinion, and extrapolations from ideas and studies elsewhere. They are not based on local data and adequate modeling.
- The terrestrial section did not appear to be closely enough based on the provincial biogeoclimatic ecosystem classification scheme, and there is an unfortunate confusion between the regional biophysical and the BEC system. According to the report’s Acknowledgements, many of the local experts on coastal forest ecology were not closely involved, nor were many local resource and environmental scientists and managers who could have helped to resolve many of the questions and deficiencies I have identified in the report. It is not clear why they were not involved, or not more involved, and why reliance was placed on scientists and consultants from elsewhere to a greater degree than appropriate for such a locally and regionally-specific study.
- The approach taken – that of modeling – was probably the only approach possible. However, the use of an inappropriate scale of modeling, the use of models based on static assessments of what are dynamic ecosystems, the failure to drive large landscape modeling with dynamic, bottom-up, stand-level ecosystem management models, and the inadequacy of the data on which the modeling was generally based renders the modeling exercise very dubious. Conclusions based thereon are equally questionable. Credit is due to the authors of the terrestrial sections of the report for recognizing that the final conclusions are based on their opinions and not on scientific analysis through modeling.
- The limitations of the modeling, noted at several places in the report, lead to my conclusion that there is at present no locally-based, scientific basis for the suggestion that 40-60% of the Central and North Coast and the Haida Gwai LRMP areas should be set aside permanently for “biodiversity” reasons. The analysis that was done is simply not able to identify on the basis of scientific

analysis and appropriate data what conservation strategy is needed and appropriate for this area: more or less than 40-60%, where it should be, and what “protection” means in the context of managing these landscapes.

- A key aspect of modeling is verification. How well do the models represent reality? Verification is especially important where there are significant social, economic and environmental implications of the models’ predictions. This is very difficult, but the best way is usually to compare the model’s output to reality on the ground. Before the conclusions of the modeling can be fully evaluated and acted on, there must be field work to verify that the models’ predictions are valid for the landscapes in question, their management and the values that are to be sustained. It is recommended that the LRMP stakeholders organize a series of field trips to examples of the different landscapes to evaluate on the ground the degree to which the modeling has captured the reality. Issues of the scales of modeling, the need for dynamic analysis and other criticisms of the modeling are best resolved standing in the forest, and not round a table in a meeting room.
- A feature of the report of particular concern is careless, ambiguous, sometimes wrong and often inconsistent usage of ecological concepts and technical terms. This semantic problem may have led me to incorrect conclusions about the report, but if this is not the case I see little basis for any assertion that the report provides a sound ecological basis for the conclusions until this aspect of the report is corrected. There are several examples of inappropriate extrapolation from studies in ecologically different areas, and of quotations of general ecological theories that are useful within the science but have limited applicability in the study area.
- In the terrestrial section of the report, the analysis appears to be based on an inventory of poorly documented present conditions (lack of data), interpreted by often simplistic and inadequate models. There is a failure to use available mechanistic models that answer the question “why”, and excessive reliance on statistical correlation models. This situation was probably forced on the modelers, and is therefore not a criticism of the modelers themselves. It does suggest that the level of modeling that was possible is quite inadequate to support the conclusions the report was asked, unreasonably, to reach
- The key conclusion from my review is that this was an unreasonable undertaking, especially the terrestrial component, which subjects the report and the study to criticisms that they should not have been exposed to. The study needs to be redone on a different basis. Available large-scale landscape models that have the capability of representing the appropriate scales for the conservation issues of interest should be driven by available mechanistic, stand-level, ecosystem dynamics and management models that are able to represent the ecosystem structures, processes and change over time that are deterministically related to these conservation issues. The analysis should not

be driven by general ecological theories that have been demonstrated to have very variable validity in some or even many types of ecosystem. It should not be driven by data and experience from remote areas unless a careful analysis demonstrates clearly the validity of such geographical extrapolation. It must be driven by local data, knowledge and experience; the re-analysis must be based much more on the local experts in government ministries, universities, resource industries and the consulting community who know the study area and its ecology.

- In the absence of reliable local data, modeling is not a reliable approach: “garbage in, garbage out” as the saying goes. I say this as someone who does both field research and modeling. Based on this key conclusion, I recommend that the government of B.C. fund a decade of critically-focused study to answer the questions that prevent this report from reaching the science-based conclusions it was charged to produce. If the government and the people of B.C. they represent wish to resolve the differences of opinion concerning land use and management in the study area, they must provide the data needed to go from the world of opinion and philosophy to a science-based situation in which people can make credible analyses of the consequences and tradeoffs between different land and management paradigms and conservation strategies to address alternative balances between different values.

1. Preamble

Forestry is defined as *the art (skill), practice, science and business of managing forest stands and landscapes to sustain an ecologically possible and socially desirable balance of values.*

The first corollary of this definition – the first responsibility of forestry – is that forestry must change as the balance of values desired by society changes. The second responsibility is that forestry should oppose current practices and policies and resist proposed changes that are inconsistent with the sociology and ecology of the new desired balance.

An essential feature of this vision of forestry is that forestry is extremely complex, involving a diverse set of social values and environmental considerations. Complexity is a feature of ‘wicked’ problems – problems that do not have a single identifiable solution. There is only a variety of different options as to how one could respond to the problem, each with a different balance of possible outcomes. In the early 1300’s, William of Occam asserted that one “*should not posit complexity without necessity*” or “*as simple as possible, but as complex as necessary*”. This was echoed by Albert Einstein when he said

that theories and actions should be “*as simple as possible, but no simpler*”. Both these assertions emphasize that complex problems require complex approaches to their solution. In the context of forestry and conservation, this notion is reflected in Aldo Leopold’s essay “The Land Ethic”:

“The evolution of a land ethic is an intellectual as well as emotional process. Conservation is paved with good intentions which prove to be futile, or even dangerous, because they are devoid of critical understanding either of the land, or of economic land use.”

The Province of British Columbia has developed the LRMP – Land and Resource Management Planning – process as an approach to the “wicked” problem of land use and forest management planning. The LRMP process is designed to address the complexity of values, resources, environmental characteristics and human needs and desires. This review examines one of the several reports commissioned to contribute to that process – the one dealing with conservation issues, an area of forestry that has grown in prominence in response to the increased public concern about this aspect of forestry.

There are many merits to the LRMP process. It is a democratic process that involves many different stakeholders, it addresses multiple values, it seeks consensus or at least an acceptable outcome, and it addresses geographical areas that are sufficiently large to assess a variety of social and environmental issues, ranging from regional resource supply and employment to species conservation. However, a difficulty that accompanies the analysis of large areas is that we are limited by our analytical techniques, computer power, the availability of local data, our ability to cross spatial scales from the detailed biology and ecology of species and local habitat considerations to the large landscape scale, and our patience. We are also limited in our ability to conduct and comprehend detailed analyses at very large spatial scales unless we use analytical tools that are specifically designed to do so.

As we go to larger and larger spatial scales, we tend to lose sight of the small scale detail that should be the foundation of large scale analyses. Large landscape analyses that aggregate or average across very different ecological conditions at smaller scales become very questionable. We may also lose sight of that critically important measure of biodiversity – temporal diversity. At smaller scales (leaves, branches, trees, stands) we are acutely aware of change. However, as we go up in scale, the local temporal change is displaced in our perception by mosaics of stands, the overall aggregate character of which may be relatively constant even though the individual stands that make up the landscape are changing. The larger the scale, the greater the impression of stability, at least on time scales of human life span and experience. This impression is supported by the necessity of using landscape models to assess issues at this spatial scale, *unless* we drive these models with time-sensitive, dynamic, stand-level, process-based, ecosystem management models. We must also never forget that landscapes also change, such as when the climate changes. Stand-level change must be addressed in large landscape models which must also address issues of climate change if conservation goals are to be achieved while minimizing conflicts with other values.

Conservation has always been a value in forestry. The desire to sustain a variety of values in forests has always been the progenitor of forestry – the first great human act of forest conservation. Much of early forestry was concerned with maintaining values such as wildlife habitat for game species for wealthy landowners, the supply of potable water and/or steep slope protection. As wood became a critical strategic and industrial material and as society increasingly depended on wood products for the necessities and desires of life, the focus changed to sustaining timber supply, employment and wealth creation. As the importance of our biological and ecological legacy was recognized, conservation of many different measures of biological and ecological diversity and of ecosystem processes has become a major focus of forestry. The LRMP process should reflect the current balance of desired values, including conservation values.

This review will start by making some general comments and recommendations, and then address specific comments and discuss some of the technical details of the ESA Report. It is assumed, as noted in the report, that this is but one of several inputs to the LRMP process. However, my review is limited to the important values considered in the ESA report. I will conclude with some specific recommendations concerning the science on which this report is based. The report should be based on both social and biophysical sciences because land use decisions and sustainable forestry cannot be guided solely by the biophysical sciences. The report appears to be largely a biophysical scientific endeavor, but in the final analysis the acknowledged inadequacies of the current state of biophysical knowledge in the study area appear to lead the report back into the realm of social sciences and value judgments. The conclusions about terrestrial areas to be set aside for conservation appear to be based largely on the opinion of the conservation scientists involved; opinion based on conservation work done outside of the study area and the more limited work done within the study area. As a biophysical scientist I am not qualified to evaluate the social aspects of this report, and I will confine my remarks to biophysical issues.

In making my comments, questions and suggestions, I recognize the enormity of the task the ESA Team was charged with considering the time and data constraints they had to work with. I defer to the experts on the ecology of focal species, although I sometime pose questions and seek explanations for things I have not understood or which my lesser knowledge leads me to question. Where statements are in conflict with my more limited knowledge, I seek clarification, documentation or justification. I am sensitive to the difficulties involved in the work they have done, and recognize that the report constitutes a very useful first step, but only a first step, in trying to answer the question of how to balance the important conservation issues in the study area with other important issues that the LRMP must consider. As in all journeys, the first step must be followed by others if we are to arrive at our desired destination. The response of the LRMP table to this report will hopefully be sensitive to this.

This is a large and complex report. Delays in my access to it and the resultant short time available for the review due to prior commitments may have resulted in my missing information and/or misinterpreting some sections. Where this is the case I offer to the

authors my apologies. However, the report should be capable of being read quickly and without misinterpretation or ambiguity of meaning, and I did not find this to be the case in some sections. In several sections, it appears that provincial experts who could have made major contributions were not involved, or not involved to the extent that they should have been. Considering the importance of the report and the very considerable effort involved in such a complex task, this is surprising. My suggestions are made in the spirit of trying to improve it so that it is less susceptible to such errors and can stand as a scientifically credible statement of the current knowledge of the conservation issues in this vast, rugged and inaccessible section of coastal British Columbia.

2. General comments

This report was a very ambitious undertaking, pushing the limits of our knowledge and local data bases. While I have a variety of questions and suggestions, I recognize the contribution that this report makes and the limitations that were imposed on the ESA Team by the time and spatial scales of the project. However, I have some suggestions concerning other methodologies that might have been used, some criticisms of some aspects of the methodology that was used, and suggestions for further work that is needed to improve the confidence level in the conclusions and the overall report. Until these are addressed I would consider this a useful draft work in progress and not a definitive statement.

I found some of sections of the report to be well written, easy to read and follow; other sections I had more difficulty with. Most of my questions and suggestions for editing and clarification are in the sections that cover topics that I am most familiar with (terrestrial ecology). Whether or not my restricted knowledge of other technical topics (freshwater and marine) has biased my evaluations of the sections that cover these topics must be judged by technical reviewers more expert in these topics than I am.

The following are general comments on the entire report:

2.1 The debate over forestry and land use has been and continues to be plagued by the use of poorly defined terms, and the use of terms for which there is a variety of possible interpretations and public usage but without an explicit statement of the way the terms are being used. This problem also occurs in parts of this report. I will address specific examples of this problem in the next section.

Recommendation #1 is that wherever a technical term is used for the first time, there should be an inset box with a definition of the term and the context in which it is being used.

Inset boxes are often easier for the reader to use than microscopic footnotes or a glossary at the end, although either of these is better than no definitions or an incomplete set thereof. These definitions must be based on the best available contemporary science, and the next draft of the report should be reviewed by the appropriate technical experts to

ensure that they are correct. Inconsistent and ambiguous use of certain technical terms reduces the clarity of the report and confounds some of the interpretations.

Communication of ecological knowledge is an important part of the science of ecology, so in reviewing the science in this report I will comment on the use of terminology and ecological concepts where I feel that attention to this issue is needed.

2.2 Conservation is an activity that involves a variety of spatial scales. Some conservation issues are related to individual trees or stands (definition of *stand* is required). Others must be examined at the landscape level (definition of *landscape* is required). All these issues require that both our classification of land for the purposes of conservation analysis and planning, and the spatial scale at which we conduct planning analyses, respect the spatial scales of the conservation issues being addressed. The many measures of biodiversity in forest ecosystems can be evaluated at several spatial scales – conventionally three (stands, local landscapes involving gradients of soil conditions, and large landscapes that involve gradients of climate) – and conclusions with respect to conserving any one of these biodiversity measures are generally sensitive to the scale at which the evaluation is done. The spatial resolution used to evaluate terrestrial ecological and biological diversity and the associated conservation issues appears to have been too coarse to give the report the scientific credibility it needs.

Recommendation #2 is that the analyses and the conclusion drawn from them should be subject to analysis of their sensitivity to the spatial scale (500ha units) at which the analyses were conducted.

The use of a 500 ha minimum resolution in the SITES model assessments of terrestrial values is too coarse to address many conservation and forest management issues. This level of resolution places significant constraints on the confidence a reader can have in the analysis results. It overlooks the trends in forest management over the past decade in which the spatial scale of forest disturbance has been significantly reduced, and now regularly incorporates elements of mature forest structure in stands at a scale of less than a hectare up to a few hectares or tens of hectares – so much less than the 500 ha resolution of the analysis that it throws considerable doubt on some of the analyses and conclusions drawn there from. The analyses also do not appear to incorporate changes over the past decade in timber harvesting techniques that involve significant reductions in roading (e.g. helicopter or long-line cable yarding), and the practice of de-roading harvested areas. The 500 ha scale of the analysis make it difficult to account for such changes, which have important implications for conservation issues. The fact that scales smaller than this are analytically difficult because of the large area of the study is not relevant if sustaining desired conservation values in balance with other values is the main goal.

We must not compromise the design of conservation and resource management by the limitations of tools and techniques. Our environment is too important to let this happen.

2.3 My reading of the report suggests that in some cases the terrestrial classification systems used are not satisfactory. The use in the terrestrial part of the report of a coarse spatial classification (the Ecoregional biophysical system) that lumps a wide diversity of ecological conditions and forest ecosystem types is not appropriate as a basis for analysis of those conservation issues that have a strong local habitat or ecosystem type component (which is most of them). While the preferable biogeoclimatic system is mentioned, the terrestrial sections of the report appear to either confuse the two systems or to confuse the reader as to where and for what purpose each system is used. The rationale for, and the specific roles of, the different systems in the analyses need to be clarified. The need for, and application of, additional systems of classification other than the world class biogeoclimatic classification system that is the legally-required ecological foundation for forest management in B.C. is not adequately explained.

Recommendation #3 is that the systems used to classify spatial ecosystem diversity (ecological diversity and biological diversity) and spatial variation in measures of biological diversity in the study area be reexamined to ensure that they are suitable for the purposes for which they are being used, and that the use of each system is made explicit and reasons for its use explained.

An example of the type of critical error that can result from the use of an inappropriate classification scheme in a report such as this is the media coverage of the report that cites the entire study area as “temperate rain forest”. This level of gross oversimplification of the ecology of this ecologically diverse region of B.C. emphasizes the need to base the terrestrial section of the report on a classification that accurately reflects this diversity – the BEC system.

2.4 Models are simplifications of reality. Unless they simplify the complexity of the real world they are generally limited in value. As we simplify our representation of reality, we are forced to make simplifying assumptions and to extrapolate from limited data. The report is generally careful to state the assumptions used in the necessarily simplified models that are used and makes some general disclaimers and cautions about the very limited data on which the large scale analyses are based, and from which important conclusions drawn. Normally in modeling one undertakes sensitivity analyses to test the implications for one’s conclusions of the assumptions, data limitations, and the scale at which analyses are done. The ESA is clearly an enormous undertaking, and time and resources undoubtedly limited the extent to which sensitivity analyses could be undertaken in addition to those that are reported.

Recommendation #4 is that the process of analysis be continued after the interim report is tabled (and the report I reviewed can only be considered an interim report considering the enormity of the task the Team faced) in order to further test model sensitivity to assumptions, some of which appear to be questionable There should also be a test of the robustness of the overall modeling framework and tools that were used, and a careful reconsideration of the modeling approach that is proposed for the future (see Recommendation #5).

Strong caution should be exercised and appropriate warnings given about drawing firm conclusions from the interim report based on concerns about the nature of the modeling approach used. This recommendation reflects the note in the report that some of the major conclusions about terrestrial conservation needs are not data-based because of lack of data and appropriate and reliable models.

A leaked copy of the draft report has already been cited in the media. Incorrect conclusions have been drawn and reported about what the report says and supports scientifically. This is not a criticism of the report or the Team that produced it, but points out the need for stronger statements about the need for caution in drawing firm conclusions at this time. A clearer statement is needed to separate science and data-based conclusions from opinions, and to separate conclusions and opinions drawn from studies, experience and ideas derived from ecologically different areas from conclusions and opinions derived from the study area.

The results presented in the report are most probably sensitive to the scale at which the analysis was done. While the Team included some sensitivity analysis in their modeling experimental design, it is my impression that much remains to be done before readers of the report can gain a clear understanding of the robustness of the conclusions.

2.5 One of the defining features of forest ecosystems is that they are dynamic, functional systems that change over time under the combined influence of external disturbance processes (human and non-human caused) and internal ecosystem processes of ecosystem development. Most conservation issues in temporally variable ecological systems cannot be addressed adequately in snapshot or short term evaluations of inventories of current ecosystem values and conditions. For example, all measures of biological diversity change over time (the temporal pattern of change constitutes *temporal diversity* – one of the most important measures of biological diversity) in all forest ecosystems, the degree of temporal change depending on the particular measure of biodiversity, the scale at which it is evaluated and the type of ecosystem.

The present focus on existing ecosystem conditions, apparently without adequate consideration of ecosystem processes, dynamics and change over time, may have led to conclusions that are sensitive to this static focus and may therefore be in question; conclusions drawn from them may be incorrect.

Recommendation #5 is that the SITES analyses should be redone using stand-level models of forest ecosystem dynamics and management to drive the landscape level model(s) on which the terrestrial analyses are based. The stand-level ecosystem management models used should incorporate our best current understanding of the causes, processes and products of forest ecosystem change over time at stand and landscape scales in response to “natural” and human-induced disturbance.

The lack of such analyses creates significant questions about the definition of human impact and its significance for conservation that is used in the report. The lack of any modeling of the response of ecosystem processes to “natural” and human-caused disturbance and to climate change raises questions about the robustness of the analyses. A major shortcoming in forestry in the past, and sometimes persisting today, is that allowable annual timber harvests (AAC) have been based on timber supply models that are aspatial and are driven by stand level growth and yield models that are insensitive to changes in key ecosystem processes that result from human and natural disturbance. OAFS – operational adjustment factors – are sometimes used to address some of this shortcoming, but OAFS are experience-based and cannot therefore address future conditions for which we lack experience. This must be done using process-based modeling. Aspatial models are generally being replaced by a modern generation of spatial timber supply models, but in many cases these are still driven by inflexible, “historical bioassay” stand growth and yield models. Conservation planning cannot be allowed to repeat this error. To its credit, the Team has used a spatial model (SITES), but it appears to have used an inappropriate spatial scale, and has apparently made no attempt to put a temporal dimension to the analysis. This is simply unacceptable as the basis for planning dynamic local ecosystems and landscapes. For the analyses and the conclusions to be scientifically credible, the impressive assemblage of data, knowledge and experience presented in the report should be re-analyzed using more appropriate ecosystem models.

This re-analysis should reflect that fundamental maxim of science known as *Occam’s razor*; “**Do not posit complexity without necessity**”, which is generally interpreted to mean that theories and models should be “**as simple as possible, but as complex as necessary**”. This was echoed by Albert Einstein when he said that theories (and by implication models) should be “**as simple as possible, but no simpler**”. A more modern admonition to incorporate adequate complexity in our approaches to ethical and effective conservation is found in the writings of that great conservationist Aldo Leopold in his critically important essay *Land Ethic*: “**The evolution of a land ethic is an intellectual as well as emotional process. Conservation is paved with good intentions which prove to be futile, or even dangerous, because they are devoid of critical understanding either of the land, or of economic land use.**”

These quotations strongly suggest the use of more ecologically-based analyses using process-based ecosystem management models of adequate complexity. Rauscher (1999. *Forest Ecology and Management* 114:173-197. Ecosystem management decision support for federal forests in the US: a review) notes that most ecosystem management decision support systems fail to address ecological and management interactions across multiple scales, and are poorly equipped to simultaneously consider social, economic and biophysical issues. This important article would be helpful background for any reader of this report.

These ideas should be borne in mind both by the authors of the ESA report as they undertake re-analyses and by the entire LRMP team as it seeks an ethical and balanced solution to the question of land use in the Central Coast and North Coast LRMP areas.

2.6 Generally, the focal terrestrial species modeling appears to have been done using the most currently available models and the latest, albeit limited, data and knowledge. For most of the terrestrial species there is a credible discussion of the strengths, weaknesses and uncertainties in the work. Much of the modeling is based on data and theories that are very recent and therefore have not yet stood the test of time. Lack of data appears to have limited the ability of the Team to do spatially-explicit modeling. The models generally appear to have been applied across large areas, without consideration of their applicability in biogeoclimatic zones and variants other than those from which the limited calibration data were collected, and under the different climates and soil conditions that this implies.

The history of timber resource modeling at the landscape scale has been one of a spatial modeling that often resulted in sub-optimal management planning. Conclusions based on models that averaged over large areas and failed to consider spatial diversity in resource values and their availability (operability) frequently resulted in significant errors in forest management planning and non-sustainable levels of harvesting. This issue has increasingly forced government agencies and forest licensees to do spatially explicit modeling. While the ESA analysis is based on a spatial model (SITES), this appears to have been driven by wildlife models that have not yet been developed to adequately incorporate the ecological diversity across the study area, rendering their spatial predictions questionable and in need of validation.

Recommendation #6 is that the analyses in the next phase of modeling should be redone with focal species models that include information from BEC zones, subzones and site types where this is appropriate and where this is not already captured in the models, and that the generality of the models across BEC units should be assessed.

Although the results are presented in spatial form (maps), if the models that drive the maps are not sensitive to significant ecological and biological spatial variations, and if the analyses and maps are not at a spatial scale that represents ecologically and biologically significant spatial diversity, the maps are probably not very reliable as spatial models.

2.7 There is a repetition in the report of the common and persistent error of confusing *ecosystems*, *biotic communities* and *seral stages*; confusing *ecological diversity* (a fairly permanent characteristic of the inorganic aspects of the environment) and *biological diversity* (a dynamic and relatively transitory set of measures of the organic component of the ecosystem); and confusing *present condition of the biotic community* (e.g. community age, the seral stage and the phase of that seral stage) with *ecosystem type* (based on edaphic or climatic climax community potential that reflects ecological diversity). My response to this problem relates to Recommendation #1 – the need for definitions.

Recommendation #7 is that the authors review contemporary concepts of succession, ecosystem terminology, “old growth” terminology and other similar terms to ensure that the way these terms are used in the report is logically consistent

and unambiguous, and consistent with modern concepts about these ecosystem processes and conditions. All these terms and the manner in which they are used should be defined and made explicit, respectively. Assistance should be sought in this regard from the talented pool of local forest ecologists in the BC Ministry of Forests, the universities and the B.C. forest ecology consulting community.

While the Team of writers and reviewers is impressive, I am surprised at the lack in the listing of major authors of several recognized British Columbian authorities on forest ecosystem sciences, on the ecology of the central and north coast, and on forest management and silviculture in this area. It is my impression that although the writing Team was strong in some aspects of conservation, it was weak in some other important areas and that the report would benefit from consultations with appropriate local experts (B.C. forest scientists) in the revision stage. While I have no basis on which to doubt the work of out-of-province consultants who were contracted to be involved in the analysis and report, I am very surprised that the abundant talent in the environmental sciences and forest ecosystem consulting community in coastal BC was apparently not used as members of the primary Team or to strengthen the report.

2.8 It is asserted: 1, that this report is unique, and 2, that it employs cutting edge techniques. The former assertion is undoubtedly correct because of the sheer size of the area and the incorporation in the report of terrestrial and both freshwater and marine aquatic ecosystems. The latter is challengeable, at least for the terrestrial component (I am not qualified to judge the Freshwater and Marine sections of the report in this respect). Analysis of forest conservation and management issues at the temporal and spatial scales that should be addressed in the report should involve landscape resource supply models or landscape-level modeling approaches (including simulated annealing-type as used) driven by stand-level, multi-value, ecosystem management and ecosystem-level succession models that represent key ecosystem processes explicitly. Failure to employ such tools (which are locally available and have been used effectively elsewhere in BC for some years to assess conservation issues) challenges the claim to cutting edge in the terrestrial section of the report. The landscape tool used is less than cutting-edge because it is not linked to stand-level, process-based, ecosystem-management simulators. The terrestrial focal species models appear to be the best available, being based on the latest data. However, there appear to be significant limitations in some of them because they are apparently not (based on the descriptions in the report) sufficiently sensitive to ecological variation across the study area.

Recommendation #8 is that the useful first step taken in this report of the complexity of the conservation issues in the study area should be developed further, and that in this further work the Team should use dynamic, ecosystem-process driven tools. This further work should ensure that the focal species models are sensitive to the ecological variation expressed in the BEC system of forest classification.

It is not clear why the Team chose the landscape model they did rather than alternatives of the type I have suggested. The strengths of the SITES model and its superiority to

other approaches that could have been used should be made explicit. The reasons for the choice of SITES rather than the available alternatives should be explained. It should also be made clear why the study Team proposes to use a version of the QUEST software for future analyses. This is unsuitable in its present form for the work that needs to be done in the future on modeling conservation issues. I assert this because I am the person currently overseeing the forestry component in QUEST which uses a level of analysis that is heavily constrained by the heuristic objectives of QUEST, and is not suitable for the extension of the ESA report, largely because it operates at the wrong spatial and temporal scales for the conservation issues addressed in the report, as is the case for the modeling done in the report. QUEST is a very useful heuristic tool for the analysis of complex social and environmental issues over relatively short time horizons. Successfully developed originally as an urban and urban interface model, its present extension to the Georgia basin extends its design capabilities to a level at which the necessary simplification of the real-life complexity greatly reduces its value for the type of application considered here.

The focal species modeling should be modified in the next round to link it to the BEC system of ecosystem classification and expanded to incorporate ecosystem dynamics and temporal diversity.

While the marine section of the report appears to my relatively unqualified eyes to be well done, I am surprised that the internationally respected UBC Fisheries Center was not involved in the analysis, or at least in the review.

2.9. Maps can be a very useful two-dimensional model of spatial relationships. However, to be useful they need to be presented at a scale that communicates the intended information. Because of the very large area included in the report, the ESA Team faced formidable cartographic challenges. Unfortunately, the use of the chosen color schemes and the disconnect between the mapping scale and the scale of the values being mapped renders many of the terrestrial maps of limited value – pretty to look at but not easy to extract the important information from.

Recommendation #9 is that either the color schemes be changed and the number of mapping units be reduced, or that those maps that do not communicate effectively be omitted, or simply be presented in a CD from which zoom-in can provide the information needed.

3. Specific comments

I comment on those issues in the report about which I feel I have the appropriate background, knowledge and experience. Because the Executive Summary (pages 6-26) reports on much of the main body of the report, many of the questions, comments and issues I have with the main body are covered in my comments on the Executive Summary.

Page iii. Reviewers.

Why were local silviculturists (MoF, industry and university) and B.C. MoF/university/consultant/industry forest ecologists not involved as technical reviewers for the terrestrial component? While the Team is reported in the Acknowledgements to have had an excellent group of knowledgeable local forest ecologists as advisors on the BEC classification system and some plant data sets, they are not listed as reviewers. I did not see the knowledgeable local silviculturists I am familiar with listed as advisors and reviewers of the report. I feel sure that had they been included, many of the questions and issues I will be raising would have been addressed already. Similarly, the UBC Fisheries Center would seem to have been a useful source of review on the Freshwater and Marine sections.

The list of people involved is impressive, but the average reader will not know their background and training, posing difficulties for the readers if they wish to evaluate the individual expertise of those involved. I suggest that this information is added in an appendix (preferred, as there should be a significant level of information provided to establish their expertise) or in the Acknowledgements following their affiliation.

Page 6. Executive Summary.

Para 1. *“In addition to productive, structurally-diverse old-growth ecosystems...”*
There should be an inset box here defining “old growth” and discussing the use of “old growth index” (OGI) as a better term. OGI should be considered as a substitute for the very ambiguous term “old growth” throughout the report. “Old growth” is used in the report to describe a wide variety of forest stand conditions, and there are examples where adjacent sections on focal species have conflicting definitions of what old growth is. The phrase here implies that “old growth” is productive and structurally diverse. This excludes all high OGI stands that are “unproductive”, and mature forest stands that have relatively modest structural diversity but high values of the other “old growth” attributes. Did the Team limit its analysis to productive, structurally-diverse high OGI stands to the exclusion of large areas of old first-growth forest that did not fit this definition? If this was the case or the intent, how were the conservation contributions of “non-complying” “old growth” stands evaluated?

“Old growth” is not a term that is exclusively defined by any one of tree age, tree size, tree species, species of other organisms, stand structure, seral stage, time since stand replacing disturbance, or absence of human disturbance. It is a complex concept that can involve any combination of these factors. “Old growth” is a late stage in development of any seral stage, from relatively short-lived, early seral, pioneer tree species, to long-lived, shade tolerant, late seral species. Conservation biologists are increasingly dropping reference to “old growth” because of the complexity and ambiguity of this term. They are replacing it with a focus on the species composition, tree size, stand structure, snags, coarse woody debris, and the soil conditions and other attributes that are important habitat for focal and other species. Various combinations of these forest ecosystem attributes can be found under a wide variety of conditions. The focus in forest ecology

and conservation is changing from often arbitrary definitions of “old growth” to ecosystem processes and key structures that are causally correlated with conservation issues. This report should reflect this contemporary shift, which the present draft does not.

There should also be a box defining “productive”; high ecosystem net primary production; high tree net primary production; and/or high production of consumers? Does this statement imply that “high productivity, structurally diverse” old forest systems are of greater conservation value and more in need of conservation than low productivity, structurally diverse stands, or high productivity, low structural diversity stands? This needs to be clarified. The apparent focus on “high productivity” stands raises the question as to whether subsequent analyses are biased towards such stands, to the exclusion of the “non-contributing land base” – areas of lower economic production, steep slopes or other areas that are inaccessible for logging, but which nevertheless may have high conservation value. While high productivity, accessible stands are probably high priority for harvesting of forest products, and this alone may render them more in need of conservation representation than other stands, the way the clause cited above is phrased does not clarify the approach being taken, and it should.

Page 6, para. 3. line 4.

“1. *represent ecosystems across their natural range of variation ...*” It is widely recognized that discussions of Natural Range of Variation (NRV) need to be accompanied by some statement as to which historical period is the baseline for the comparison. Because of evidence of past changes in forests due to changes in populations and activities of First Nations and changes in climate, there needs to be a temporal baseline established for this comparison. Is the report referring to Historical Range of Variation (HRV – for which period, and why that period?), Present Range of Variation (PRV – is this the “correct” range, and if yes, why is it more or less correct than the range for any other period?), or some Desired Future Range (DFRV)? There also needs to be a box defining the intended meaning of the word “natural”. Whenever NRV is discussed, there needs to be an evaluation of what this means in the face of predicted anthropogenically-induced climate change. Modeling of NRV requires then use of dynamic ecosystem models that address the impacts of possible climate change on ecosystem structure and function.

“3. *...acceptable range of variability,..*” If the goal is to conserve NRV, how does this relate to a human defined “acceptable range” There seems to be a conflict between objectives 1 and 3.

Second bullet. line 2. “*ecological communities*”. Communities of living organisms are a biological entity. Combined with their physical environment they are an ecosystem or ecological entity. Correct terminology here would say “biological communities” or simply “communities” (since all communities are biological, there is a redundancy in saying biological communities). If the desire is to differentiate non-human communities from human communities, then this should be made explicit. Human

communities are also biological communities, but different from non-human communities.

Third bullet, line 3. “(c) for which sound habitat-suitability models..” Does this mean “validated” models ? The term “*sound*” is not normally used in modeling and in reference to models. Sound by what criteria? *Validated* implies that a model’s predictions have been tested against the best available measure of reality (which may not be very good, making model validation a difficult task) and found to be sufficiently accurate for the model to be useful for the intended application.

Page 7. sentence 1. “*computerized site-selection algorithm*” If this report is intended for non-technical audiences, I suggest a change in wording or an insert box explaining what is meant here. The report is going to have to explain the complexities of the modeling exercise more clearly if it is to be understood by non-technical audiences at the LRMP table and elsewhere.

Regional Classification.

I find that the report does not do an adequate job of explaining the biophysical regional classification and justifying why it is used as well as the Biogeoclimatic (BEC) system, which was adopted by the B.C. Ministry of Forests because it works better than the biophysical regional system as a basis for site-specific decisions about sustainable management and conservation of forests. I taught a course in BEC classification at UBC for many years following the retirement of Professor Vladimir Krajina (the creator of the BEC system), including the period when there was energetic debate over which system to use – BEC or the national Biophysical Classification (called Ecoregional Classification in the report). At least part of the difference of opinion was related to questions of who would control the system of land classification in BC – the Ministry of Forests or the Ministry of Environment (the terminology of the period). The difference also relates to the different needs of these different Ministries. Forestry chose BEC because it related more closely to the local diversity of soil, topography and vegetation, and was an integrated ecosystem classification system – something that is essential for the integrated management of ecosystems and sustaining a wide variety of environmental and social values.

The biophysical system was favored by some Ministries of Environment in Canada because it facilitated independent inventory of vegetation, soils, topography and wildlife by the different departments in these ministries; the independently classified ecosystem variables were simply combined by means of overlays. The biophysical system was also favored by some because the wildlife component and interpretations of the BEC system were very poorly developed at the time. Foresters found the biophysical system much less satisfactory than the BEC system in areas with steep environmental gradients (such as in the coast mountains), although it had merits in some interior landscapes that are dominated by frequent natural disturbance and have gentle environmental gradients. There needs to be a clear exposition of the two systems, including a table that shows how

the terminology of the two systems compares and identifies the values of the two systems and where in the ESA analyses each system is used.

I believe that one of the problems I find with the Terrestrial section of the report comes from the inadequately explained, and perhaps inadequately understood and conceptualized, mixing of these two systems. For example, some of the very large units – Ecosections – from the biophysical system include several BEC zones and many BEC subzones/variants that differ greatly in their ecological character and conservation values. This has led some environmentalists and the media to refer incorrectly to the entire area as “rainforest”, whereas the BEC system identifies within one single Ecosection the following very different forest ecosystem types: temperate rain forest of various different types; bog forest, bog woodland and bog; drier montane forests; coastal, “warm” snowpack, snow-dominated subalpine forest; subcontinental, cold snowpack, subalpine forest with lower snowpack; and some small representations of rain shadow, continental, sub-boreal forest types and interior dry forest. A quick addition of the data in Table 2.4 suggests that about 35% of the entire study area is non-rainforest BEC zones and subzones. Not all of the remaining 65% is rainforest because of the large areas of coastal bog and bog forest. In some of the LRMP units the proportion of rain forest is lower. The report should make a clear statement of the size and the location of forests that actually match the definition and ecological character of temperate rainforest, and list these separately by LRMP unit. The BEC system is well suited to this task.

The hydrological, wildlife and conservation issues are so different in the different BEC units and forest types that there seems to be little value in lumping them as occurs in the Ecosections and the ecological drainage units (EDUs). Why was the BEC system not used throughout? What is the justification for horizontal stratification (e.g. maps 2 and 3) rather than the vertical *and* horizontal stratification provided in the BEC system? What is the basis for the hard boundaries shown between adjacent Ecosections and adjacent Ecological Drainage Units in these maps? This needs to be explained and clarified. If the subsequent analysis does not depend on these questions, then why not omit these maps? If the Ecosection classification *is* used as a basis for the analysis, then a much more detailed discussion and justification is needed.

There is inadequate explanation of the other two classifications that are mentioned. I looked at the classification developed by an Oregon consultant, and could not understand how and where and why it was used in the report. Its inclusion in the report was hard to understand when we already have a world class, locally developed and applied system (the BEC system) that is the legally-required ecological classification foundation for the management of forested landscapes in BC, and will by law be the basis of implementation of the LRMP recommendations in the coastal forested landscapes.

Page 8. para 2. This is a useful statement of the very limited data base available to the analysts for many of the focal species models. It would be useful in this summary to emphasize the uncertainties that this poses. Many readers would not understand “post-hoc analysis” This needs to be de-mystified, and its contribution to dealing with uncertainty explained in simple language.

Terrestrial Ecosystem Representation.

Para. 1. line 2. It seems to me that the biophysical system has some value as a regional classification system, albeit with the limitations I have discussed above. However, I do not see it as a useful system by which to define local terrestrial ecosystem types and their present and possible future condition – issues that are very important in conservation. Perhaps the use of the biophysical approach has contributed to the sense of static assessment that the report gives.

Line 5. *“two independent classification systems”* It is not clear what one of these two is. Please clarify. Also, please describe how the Oregon system is applied in the analyses and why it was decided to develop an additional system for this area and the purpose of its use in the ESA when we already have the very well developed BEC system? There may well be good reasons for having done this but they were not apparent from my reading of the report.

Para. 2. line 3. *“the assumption that site productivity correlates with areas that have climax, or old growth, ecosystem characteristics”* is untenable and must be changed for the report to be scientifically credible. There needs to be a box inserted with definitions of ‘climax’ and “site productivity”. In northern forest ecosystems, especially in very humid or wet systems, forests that have gone a long time without significant ecosystem disturbance (climax stands?) generally have reduced ecosystem autotrophic productivity, and especially tree productivity. In fact, in many northern, cool, very humid and cold ecosystems, closed forest is not the climax community – sphagnum muskeg and ericaceous woodland is the more competitive plant community under conditions of declining soil nutrient availability, drainage, aeration and/or temperature. This is the successional trend identified for much of the cooler, wetter parts of the coastal sections of the study area by Allen Banner and other local Ministry of Forests field ecologists. The extent of such non-forest and woodland systems in the parts of the study area that have little fire and landslide disturbance is noted in the report, yet the whole forest is referred to as “rainforest”. There is certainly a cool, temperate, rainforest climate in some of the study area, but closed temperate rainforest exists in many of these ecosystems only as a consequence of periodic, albeit infrequent, disturbance. If the assumption quoted above underlies the subsequent analyses, I would have serious concerns about the validity of those analyses. Table 2.4 should identify which subzones and variants of the Coastal Western Hemlock Zone are predominantly non-forested or bog-woodland to correct the area that is true “rainforest”. And the table should be separated into Central Coast, North Coast and Haida Gwaii LRMP areas. It should be noted that “rain forest” is more a function of climate, the associated species and ecosystem processes than tree age and size and stand structure. Young, even-age rainforest is every much a rainforest as is a high OGI old rainforest. These different rainforest seral stages differ in species, structure and ecosystem process rates, but they are all rainforests if they are dominated by native tree and other species typical of the rainforest climate.

Para. 2. line 6. “*we also assumed a relatively low level of natural disturbance*”
Considering the Natural Disturbance Type (NDT) Classification map of BC, someone not familiar with these forests could be forgiven for making this assumption. However, most BC forest ecologists now accept that the first version of the NDT map needs extensive revision. More detailed knowledge of the forests of the study area suggests that the disturbance regimes vary greatly between different BEC zones and subzones within an NDT unit, and on different aspects, slopes and geological substrates within any climatic regime. Natural mass wasting, defoliators, other insects, diseases/parasites and wind disturbances may be relatively infrequent on a human time scale but are very important processes that shape the locally variable character of these forests. Anyone familiar with the CH/HA phenomenon in the Coastal Western Hemlock zone would know that relatively small to medium scale disturbance in these forests (sometimes related to diseases/parasites, and often interacting with wind) is closely related in still incompletely understood ways to ecosystem productivity and ecological character. The relatively frequent disturbance by wind and the less frequent disturbance by mass wasting have played a key role in the stand and local landscape diversity of these forests.

Based on these comments, I think that Map 5 is flawed in that it seems to imply that “developed” forest is not forest. Forest is forest, unless the Team wishes to alter the definition of *forest*. Young forest is as much a forest as is an old forest as long as the trees are dominating the ecosystem processes. Young and old forests have differences in structure, composition and processes, but to map them as different types of ecosystem is clearly inappropriate. The definition of “developed” seems to be an area that has >2% human-caused disturbance, irrespective of how much, how severe and how long ago disturbance occurred. The implication seems to be that once disturbed (developed), the forest landscape will remain forever in this condition, a view that excludes the dynamic nature of forest ecosystems, their post-disturbance successional development, and the time dimension of biodiversity (temporal diversity) and conservation.

From page 49 it would seem that this error may have come from an interpretation of the Sierra Club mapping of logged areas (colored yellow on some maps I have seen), and the assertion by some environmental groups that second growth forests are profoundly and inevitably different from first growth (unharvested) forests. Some even assert that second growth forests are all “plantations” and not even forests. This is inconsistent with the current state of silviculture and forest ecological science, and if the science of the report is to be acceptable such poorly informed interpretations should be omitted. I suggest a modification to Map 5 to present second growth forest (“natural” and human-caused) as light green rather than yellow. While yellow may be a cartographic convenience, linked to the interpretation of “developed” that is used in the report, and linked to the assertions of some people that second growth forests are “biological deserts”, I believe that the choice of yellow for second growth forests communicates an inaccurate and scientifically wrong message. To refer to these frequently diverse and productive areas of native second growth forest in terms that suggest that they do not recover a variety of desired values over time following disturbance if managed appropriately is not consistent with what you can observe on the landscape. The present “forested landscape”, some of which is non-forested coastal bog and woodland, should be separated into: “closed first growth

forest” (forests of any age not significantly disturbed by human action, including First Nations), “closed second growth” (forest of any age that has been altered by human actions) and “natural non-forest communities”. If this cannot be mapped at the 500 ha polygon unit the study used, then I have serious concerns about the utility of the maps, and any analyses based thereon. Models – maps and computer models – that do not represent reality sufficiently to meet the objects of the modeling are not useful.

Para. 2 line 7. “...represent intact areas” This needs to be defined. I will return to this term later. The entire sentence (the last 3 lines of this paragraph) in which this phrase is found is hard to understand – are there missing words? It should be restructured.

Para. 3. Why does this refer only to zones, subzones and variants? There is often greater variation in ecosystem structure and productivity between different ecosystem types along local soil and topographic gradients within a subzone than between comparable sites in adjacent subzones. This is the concept of *site series* – sites with similar species composition, soil conditions and expected climax biotic community in different climatic areas, but found at different locations along local soil/topographic gradients within the different subzones/variants.

Why in this paragraph did the Team elect to assume that an area had been logged when there was discrepancy between the different data sources? In the case of irresolvable conflicts, would not a 50/50 allocation to logged/unlogged have been more logical? But where there was doubt, why did the Team not go to the local licensee or to the MoF data base for the area in question to resolve the uncertainty? Or use high resolution satellite imagery.

The intended meaning of “*Ecosystem type*” as used here should be defined. What is meant by “...*historical abundance of ecosystem types* ..”? Short of climate change and/or massive soil erosion/mass wasting, ecosystem types are relatively stable landscape features – only the age and species composition of the biotic community of the ecosystem type change. Different stand ages, seral stages and/or seral stage phases of a given ecosystem type are referred to as *ecosystem phases*. Thus, except in areas of severe mass wasting, the ecosystem types will be similar to the recent historical types, even if the biotic community – the ecosystem phase – may be different. This comment applies to line two of the next paragraph as well. Over longer time scales (several centuries to millennia), ecosystem types may change because of successional changes to soils, or due to past non-anthropogenic climate change.

Para. 4. Was the SITES analysis based on the entire landscape area, the entire forested area, or just on the “contributing forest land base”. Was the “non-contributing land base” included? There is no discrimination in the report between old style exploitative clearcutting (that in many cases resulted 80-120 years later in relatively high old growth index (OGI) maturing stands), low OGI younger stands that were “carefully” logged under government regulation over the past 50-60 years, and the more recent variable retention harvesting that retains a component of old forest structure within second growth stands. To truly reflect the conservation contributions of these different

forest conditions, the analysis should be sensitive to the way in which the stand has been disturbed, and should be conducted at a spatial scale that captures these details.

Page 9. line 3. “*historical area*”. What does this refer to? I do not follow the logic of the equation.

Para. 2. line 2. My experience, and that of UBC’s emeritus Professor of Dendrology Dr. John Worrall, do not support the statement that there are at least 25 species of conifers in the coastal rain forests. It is not clear that there are more than 25 conifer trees in all of the study area or in any one of the Ecosections. There certainly are not this many tree species in the rainforest BEC units, which are rather tree species poor, or in any other individual BEC unit. This number needs to be referenced and justified. Where did it come from? Again, there is a need to define *rainforest*.

How are “*inventory group types*” defined and identified? To my knowledge inventory groups are not the same as ecosystem types or “ecological systems”.

Goals should be set by BEC site units, not Ecosections.

“*This method allowed us to represent a full range of ecosystem types without the need to know exactly which ecosystem or community type is present*” I do not understand what this is saying. It does not make any scientific sense to me. However, this strange sentence does appear to explicitly accept that there is a difference between an ecosystem type and a community type – something that is missing earlier in the report.

Terrestrial focal species

Generally this section raised far fewer questions than the previous section. However, many scientific and management-related questions remain.

Marbelled murrelet

What do we know about the implications of VR harvesting for marbled murrelet? Is the assumption made here that all logging is progressive clearcutting? What is the implication of a mosaic of harvested contributing land base of various ages and size/shape distributions, and non-harvested non-contributing land base? How accurately does a 500 ha analysis polygon capture the biology of this species? Why did the model not capture the known high elevation nest sites even when the elevation criterion was relaxed? If the model cannot give an indication of habitat quality, how useful are the spatial analyses? What are the key habitat elements for the murrelet, and are these captured by the model and the scale of analysis? There seems to be the implicit assumption that habitat is only provided by “undisturbed” and “old growth”. What is the murrelet use of areas of naturally fragmented habitat? Was there any analysis of habitat produced by management? What features of habitat could not be sustained in appropriately managed forest? What is the role of avian predation in defining habitat?

Northern Goshawk

Page 10, para. 2. What are the key structural elements selected by goshawks, and can these be sustained in partial harvest systems? What is the implication of VR for this species? Does it use naturally fragmented habitat? The definition of desired OG habitat is high canopy closure and a clear understory. This is in conflict with the structural attributes defined by some definitions of OG, and with the definitions of desired OG habitat for some other focal species (e.g. OG has multiple canopy layers, many gaps, and well developed understory). Does this discrepancy allow for the fact that there is a wide range of stand structures and ages that are included in the definition of OG within the report? Can the desired structure be created in second growth by appropriate management? Does the analysis assume that once an area is harvested it will never again be goshawk habitat? Does the 200 m buffer around all roads relate to the biology and behavior of this species?

Sitka Black-Tailed Deer

The importance of snowpack depth for this species is well known. However, if I am correct much of the work on this topic was done on eastern Vancouver Island in areas that periodically get very heavy wet snowfalls and deep snowpacks. Is this experience relevant for all of the study area? What is the spatial and temporal distribution of snowpacks and heavy snowfall events in each of the BEC units of the study area? Was this variability accounted for in the winter range model? What has the trend in deer populations been in areas on Vancouver Island that have been heavily logged and subject to periodic heavy snowpack years? What is the role of this snow factor relative to hunting and natural predation? Does the experience on Vancouver Island and elsewhere have relevance for the study area? Will harvesting increase deer abundance by increasing summer habitat, and what are the implications of this for winter habitat? Para 3 addresses some of these issues. So why not address them through the BEC system? Apparently the inability of the model to address the comments of expert reviewers led to its not being used. What is x% in the last line of the last para? Please explain the post-hoc analysis.

Mountain Goat

It is not clear how the type of habitat used by mountain goat can be identified in a landscape model that has a resolution of only 500 ha? I understood that the rocky areas they often select can be much smaller than this?. Why does the CIT-wide potential winter habitat model only account for 65% of the observed occupied area (if my reading of the report is correct)? If the accuracy is so low, what are the confidence levels in the CIT-wide model?

Grizzly Bear

Page 12, para. 2. This seems to suggest that “light touch” (variable retention?) harvesting is compatible with grizzly bear habitat. Is this in agreement with the designation of “developed forest” which appears to be a permanent designation in the context of the analyses?

Freshwater Focal Species. Page 14.

Tailed Frog.

Para. 1. “*Adult tailed frog abundance is positively correlated with the percent of OG forest in a watershed.*” What is the definition of OG here? What are the features of “OG” that contribute to this relationship? Is the correlation based on a comparison between unharvested high OGI forest and freshly harvested areas? What is the comparison between tailed frog and harvested watersheds that have adequate stream buffers? What is the relationship with VR harvested areas? What about the relationship with young to mature high OGI areas? The last para. appears to answer several of these questions to the effect that the frog will be sustained if adequate forested buffers are left. But this seems to contradict para. 1? This needs to be clarified.

Pacific salmon and steelhead

Interesting data that I have seen before. Has there been a careful study of the declines by watershed, correlating them with the extent, type and date of timber harvesting? This is not reported. Studies of this type in the PNW US did not support any simple relationships between salmon declines and timber harvesting. This analysis needs to be done to inform any conclusions about the need to exclude appropriate “stream and fish friendly” logging from areas that are important for these fish species. In the last sentence it is suggested that changes in land use are involved in the declines. What changes are these? I was not aware that there was any large scale change from forestry to other land uses in the study area?

Setting Goals Page 18.

Yes, establishing goals is amongst the most difficult and important scientific questions in conservation planning. It one of the most difficult questions for both biophysical and social sciences – social because the goals that are set may reflect more the values, beliefs and concerns of people (social sciences) than biophysical sciences, because the latter may not yet be able to answer these questions. Even if biophysical sciences could answer all the biophysical questions, goals still represent a social science activity because they are based on individual peoples’ values.

Para. 1 last line

“*ecological integrity*” needs to be defined. This is as complex or more complex an issue than “old growth” with as great or greater variety of interpretations. It is often quoted as a mantra without any sense that its use is understood. A clear definition is required to prevent this interpretation of its use in the report.

“*as biodiversity.....increase..*” What measures of biodiversity? Temporal, structural, species richness, species evenness, functional, genetic, etc? At what scales? Some measures may decline or increase as a result of disturbance when evaluated at one particular spatial scale, but show the opposite trend when evaluated at a different scale.

Page 19. para. 1.

“Importantly, site selection algorithms, by themselves, do not address the more difficult and real-world questions concerning the area needed to maintain viable populations of species...” Agreed.

“Therefore, we were unable to evaluate the potential population viability of these species in alternative networks of reserves compared to the current network” A scientifically honest conclusion.

“ Nevertheless, we can use the results of other studies to qualitatively evaluate the ability of alternative designs to sustain populations of focal species over time” This statement requires explanation. Other studies are relevant if they were conducted in comparable climates, topography and vegetation in areas with similar species or the same species of similar genetic make up, behavior etc. For example, dwarf mistletoe of western hemlock appears to behave differently as a parasite in Oregon, the lower B.C. coast and the central and north coast, and differently again in Alaska – the same species on the same host but with very significant differences in host-parasite interactions and ecological and management implications. Consequently, it appears that experience with this host-parasite relationship in Oregon and Alaska may not be useful in many parts of our coast, and *vice versa*. And the relationship varies in different areas of the coast and in different BEC units where this host/parasite combination occurs. Extrapolation of experience between ecologically different areas must be done with appropriate caution. At best the comparisons may be only qualitative. This raises the question of the robustness for the study area of conclusions based on research done in ecologically different areas. Conservation and other aspects of forest management must be based on local knowledge.

“Sustain populations” is another seemingly simple but potentially complex issue. What is the area extent of the population? Are we talking of local subpopulations, meta-populations or some other unit? Stability is not the hallmark of local populations because of predator/disease-prey relationships and the inevitable successional change. Meta-population stability may involve periodic local extirpation. The use of this phrase here needs to be explained.

Para 2.

“Generally, most studies and experts have concluded that some degree of protection for at least 40-60% of the terrestrial lands and fresh waters would be required to sufficiently protect biodiversity in temperate regions, assuming that the very “best” and representative areas are selected” Wow!

With due deference to the authors, this sounds to me more like a toothpaste advertisement than a science-based statement. Let me explain.

“Generally”- in what % of the relevant studies that have been conducted?

“most studies and experts” – what % of the studies and experts have reached a different conclusion. Based on which surveys was this based? Why have they reached a different conclusion? On what basis did they challenge the opinion of the majority?

“some degree of protection” – what does protection mean here, and how much protection is enough. How much protection do variable retention logging and stream buffers

provide? How is this level of protection factored into an analysis that has a resolution of 500 ha? And how does the concept of protection change over a 100 year management cycle?

“40-60%” What is the origin of these numbers? What science are they based on? What accounts for the variation from 40-60%? Does this represent different ecosystem types, climates, topographies, vegetation, species etc? Does this imply that at least 40-60% of the area of “productive, old growth forest” be set aside from any human resource use? Or does this mean that 40-60% of the harvesting should be variable retention? What level of retention? What level of hard retention and what level of soft retention, with what period between first and last entries? Does this mean harvesting without roads – by helicopter? Or ground-based systems with road rebuilding and prompt right-of-way reforestation? Culvert and bridge removal? Aggregation of a frequency distribution of harvesting patch sizes into disturbance events to minimize the length of active roads and to leave large areas with no disturbance for long periods? The list of questions could go on.

Page 111 addresses several of these questions and supports the idea that there is need for careful biological and ecological classification of the key variables that contribute to the answers to these questions. It suggests that the 40-60% range is illustrated in Table 3.1. However, without going to the original technical papers this table does not provide any guidance in this respect, and the goals listed do not suggest any % of “protection” or what protection means. Rather, they suggest an outcome of reserved area; a results-based rather than a regulation based approach. The areas suggested for Grizzly bear on page 112 suggest reserves of 1 million to 4 million ha. With a study area of 11 million ha, much of which is not forest, this suggests that having more than one such reserve would require most of the area. Is this the origin of the suggestion that 40-60% at least be set aside? How does the conclusion that 40-60% should be set aside relate to the SITES runs at 30%-70% goals settings (the meaning of this could be explained more clearly – I take it to mean 30-70% of the maximum predicted potential habitat for the focal species concerned).

What constitutes the protection of freshwaters? Streams – stream order and regimen? Rivers? Lakes, wetlands, bogs? How does one decide if it should be 40% or 60%?

Threats (Human Impacts) Analysis

Page 20. What is the ecological and ecosystem process-related basis for differentiating between *modified* and *developed* watersheds? What is meant by “slightly affected” in differentiating between *pristine* and *modified* watersheds? Why is there no time dimension to these definitions? Do they mean that 100 years after a watershed has been “slightly” modified it is still modified; that 100 years after 8% of a watershed has been disturbed by logging it is still a developed watershed whereas the same watershed disturbed 50% by landslides or windthrow is still pristine? What do these different designations mean in terms of habitat for focal species and ecosystem processes?

What is the basis for a 200m buffer around roads? Is this based on local studies or studies in other areas? On what variables is this buffer width based, and where and how were they measured?

What is an “*intact watershed*”? A definition of this state should be added to the three other definitions of watershed state.

What does “*ecologically intact*” mean? How does the natural process of mass wasting affect the ecological intactness of a watershed? Does the same area and severity of mass wasting triggered by human activity have a different impact on the intactness of that watershed in comparison to a “natural” event of similar severity and area?

Is a river system in a logged watershed “intact” if its streambank, riparian zones, hydrology – regimen, quality and quantity – are within the natural range of variation?

Spatial Analysis

Para. 1. Again, does “*biodiversity values*” refer to all measures of biological diversity, and are these being considered at all scales? Are there biodiversity objectives and targets for each measure at the three major spatial scales that deal with the conflicts between some measures and scales? And how is temporal diversity dealt with in terms of objectives and targets? How do temporal biodiversity objectives fit with those for the other measures?

Terrestrial and Freshwater Analysis

Para 1. There should be an explanation of why all species are given the same penalty (page 127)? It is a reasonable decision if there is no basis for assigning different weights, but in such a case there should be a sensitivity analysis of the implications of this decision. Surely there is some conservation basis for assigning different weights?

Last line. As a terrestrial ecologist with some familiarity with streams, I would argue that terrestrial ecosystems are also very dynamic – e.g. the seasonality of litterfall, fine root dynamics, soil microbial activity, wind and snow disturbance, slope instability and much more. Certainly some of the time scales of variation in terrestrial ecosystems are longer than those of some of the variations in some stream systems. Perhaps the origin of this statement is the fundamental error in the terrestrial section of this report – the apparent ignoring of the important temporal variation in terrestrial ecosystems. Much of the terrestrial report appears to present a snapshot, fixed-in-time analysis.

Page 74. I like the section on classification and its importance. It appears that the Freshwater section has attempted to integrate biophysical and biogeoclimatic classifications more completely than in the terrestrial section. Is there a reason for this difference? Does it reflect more of an ecosystem approach in the Freshwater group and more of a vertebrate approach in the terrestrial group?

Page 76. How was the total number of “freshwater ecosystems arrived at? How do these relate to stream orders?

I like the 2.3.1 and 2.3.2 sections.

Page 77. Last line. Need to define “old growth” in this context. I suggest that the CIT Team looks at the papers in Environmental Reviews Vol. 11, Supplement 1, 2003 and Forestry Chronicle Vol. 79, no.3, 2003. Also the writings of Andy MacKinnon and others in the Coastal Chronosequence Symposium Proceedings (Trofymow and MacKinnon 1998). It is surprising that much of the literature about old growth forests from B.C. is not in the reference list.

Page 78. Para.1. All forests dampen microclimatic extremes. Why is this attributed only to “old growth”(undefined)?

Para.2. There is no discussion of the significance for tailed frogs of the retention of riparian vegetation – shrubs or trees. There is the implication here that ANY harvesting in a watershed threatens the tailed frog. Evidence should be presented that any harvesting in a watershed is of conservation concern for this species whether or not stream buffers of some type are left. There is also no discussion of variations in the threat to this species from logging according to slope, biogeoclimatic subzone (related to precipitation and stream regimen), geology and other variables. Of the threat to this species, what proportion is attributable to roads and what to the cutting and removal of trees? What are the implications of variable retention harvesting, helicopter logging, long-line full suspension logging? For how long after harvesting is there a threat to habitat, and how does the persistence of this threat and of habitat damage vary with topography and the many other variables? How does this species respond to natural disturbance, and how quickly are depleted areas recolonized after natural or human-caused disturbance? Clearly there are many questions, and the Team cannot be faulted for being unable to answer them all. They can be asked why none of this variability is raised in the report, and why the reader is left with an essentially static assessment.

2.3.3.2. Methods. It would be helpful to know how these critical habitat variables are related to the processes that determine habitat suitability. Without some indication of why these topographic limits are related to critical habitat, it is hard to have confidence in the apparently purely physical relationships that are presented and the simple application of the same set of criteria everywhere.

Page 79. 2.3.3.3. Results and discussion. It would be useful to speculate on why the model was “poorly” correlated with field data; and is a correlation coefficient of 0.6 a bad fit? Perhaps, as suggested above, the model is too simplistic. More research is suggested before the model can be used to make critical land use decisions.

The last paragraph in this section supports the conclusion that much of the analysis has been undertaken assuming logging to streambank at all locations, without adequate consideration of the role of riparian buffers of some sort, of “careful “ logging, and of reduced roading.

Page 80. Line 1. As an ecologist interested in nutrient cycling I recognize the important aquatic and terrestrial issue of nutrients transported by migrating fish, and the biogeochemical role of fish eating animals in the riparian environment, and transport away from the stream environment. However, the addition of nutrients by avian and mammalian predators of fish is likely to be most important in the riparian areas. “B.C.’s coastal ecosystems” as used here is an overstatement of the generality of this biogeochemical pathway. To suggest that the N, P and C so added to terrestrial ecosystems is “the biochemical building blocks of these ecosystems (by implication, all of coastal BC forests) is such an overstatement as to suggest a significant lack of understanding of the complexities of forest biogeochemistry and the role of atmospheric and edaphic inputs.

The long term productivity of river corridors and fish habitat is certainly affected by the nutritional consequences of abundant salmon runs where these occur. Nitrogen fixation by red alder is known to be influenced by P availability, and the P inputs from fish-borne nutrients may have an influence in determining stream-side alder N fixation. P availability to alder is influenced by soil pH. Where this is low (as is common in many of the undisturbed forests in the study area), it is unclear as to what % of the fish P would be available to the alder. Where soils have become highly humified and podzolized, largely excluding tree roots from the mineral soil, addition of “fish fertilizer” could have a very significant affect on tree and ecosystem productivity. However, if this nutrient source is as critical to these forests as is suggested here, a reduction in inputs of 50-75% should be expected to have resulted in a major decline in forest growth in the riparian zones. I am not aware of data that support this. Clearly this is a topic in need of further biogeochemical research before we know the implications of salmon run declines caused by one or more of over-fishing, ocean changes or loss of fish habitat.

Page 81. 2.3.4.2.3. Why was a two point scale used? Was a sensitivity analysis done to assess the affects on conclusions of a 3 or 5 point scale?

Page 82. 2.3.4.3. Has an analysis been done to correlate the time trends in individual fish stocks with the extent (% of the watershed), type and timing of timber harvesting in the watersheds these fish stocks? What is the correlation between stock declines and these variables? If such analyses have not been done, what data are the conclusions based on? If such analyses have been done, what scale of analysis has been done (e.g. declines correlated with disturbance in just the spawning reaches vs correlations with disturbance in rearing or migrating corridor reaches?

Page 83. 2.3.4.3.2. It would be interesting to know the correlation between salmon declines and the rate, area and type of harvesting system by BEC unit. Does the relationship vary with elevation, precipitation, snowpack, geology, slope, vegetation types, seral stage ,and phase of stand development. Correlations are a good starting point, but eventually we have to understand why. What is the “causation”? Only when we know this can we be confident that the best conservation strategy has been adopted. What

is the correlation between fish declines and changes in populations of bears and fish eating birds?

In Table 2.17, page 84, there is a need to relate the biomass trends to changes in terrestrial ecosystem habitat condition, climate, ocean fishing and oceanic temperature and other conditions in order to start to identify causes. This seems critically important for conservation planning as is suggested on page 86, section 2.3.4.4. This section notes the need to balance conservation efforts between the many different determinants of undesirable population trends.

Next to last line – what does “long-term protection mean”; maintenance of the natural range of variation, whatever this is defined to mean? Does it mean focusing on headwaters protection, riparian leave strips, better and fewer roads, etc.? It would be helpful to elaborate here.

Page 90. Why do pink salmon in the north do better than in other areas?

Overall, Section 2.3 is well written and documented. Naturally many questions remain, but this section clearly identifies many research needs before the causation of the conservation issues addressed in this section are understood to the point at which we can be confident that specific conservation initiatives will be effective in achieving objectives.

Section 2.4 Marine targets.

Page 94. 2.4.1.3. How are species defined as “*ecosystem engineers*”?

Page 97. I was pleased to see that this section deals with a shoreline habitat scale (25m) that is much more ecologically meaningful than that used in the terrestrial section (500 ha).

Page 102. Table 2.25. Do ecosections and ecoregions provide an adequate representation of different ecosystems? I think not.

Why are umbrella species different in marine and terrestrial environments? It is not at all clear to me that conservation of the critical habitat needs of grizzly bears will necessarily conserve critical habitat needs of all other species in the area. I am not a conservation biologist and am not familiar with all the literature on the umbrella species concept, but it would be useful to have a brief discussion of the concept somewhere in the report and the implications of the application of the concept in the SITES analyses.

Page 105. para. 6. The relationship between murrelet nests and mossy mats on large branches of old trees seems to be well established. However, I am less familiar with the relationship between such nesting sites and “old growth forest”. How suitable is high OGI forest with scattered suitable nesting trees such as produced by variable retention systems of management? If forest harvesting retains suitable nesting trees in retention patches, what is the time course of murrelet habitat values in such stands? Where there is

evidence of murrelet abundance decline, what is the correlation between this decline and the harvesting of trees? And how would any such correlation, if it exists, be affected by the change in harvesting over the past few decades from progressive clearcutting, to dispersed clearcutting, to variable retention logging?

Page 108. Section 2.4.2.2.3. This section is interesting and conforms to my understanding of the relationship between ecological diversity and several measures of biological diversity. It also suggests clearly the need for a spatial scale of resolution in conservation analyses that is much finer than that used in the terrestrial analyses. I recommend that a section like this be added to the terrestrial section of the report, and that this discussion be used as the basis for re-evaluating the spatial scale at which the SITES or any other modeling analysis is undertaken. I believe that this section poses a strong challenge to the terrestrial section of the report.

Section 3. Setting Goals.

Page 108. Para. 1. Line 7. Agreed – setting goals in any activity, whether in the conservation aspects or the timber harvesting aspects of forest management, requires a clear statement of goals. I also agree that, in principle, such goals are better set on the basis of experience than on theory. However, with the long time scales involved in forest conservation issues, we generally lack the experience. Adaptive management suggests that the answer to this is monitoring and modifying resource management (e.g. management of fisheries, timber, and recreation and conservation values) as experience suggests. This approach is suitable for systems in which there is a relatively short time between a change in management and a measurable response in the target value, and where the system of determinism is sufficiently simple that a change in one management variable will produce an unequivocal and measurable response. In many forest conservation issues, the response time scale is too long and the determinism too complex for such simple and desirable outcomes. One response to this situation is the construction of predictive models of sufficient complexity and temporal and spatial extent to bound the issue and permit analysis thereof. This is the conceptual approach taken in this report and I support it, as long as the models used and their spatial and temporal resolution are appropriate, and they are of the appropriate level of complexity. Not one of these three appears to be adequate for the terrestrial analyses. This does not suggest a change in the approach, but a re-analysis based on more ecosystem-process-based modeling, covering appropriate time scales (“*at least 100 years or 10 generations*” – para 3 line 4) and appropriate spatial scales (page 106). An issue that is raised when longer time scales are considered is the threat of significant anthropogenically-generated climate change. This suggests the need for climate-sensitive modeling to be undertaken to assess the sensitivity of any recommendations to plausible climate change scenarios.

Para. 3. line 3-4. I agree. This sentence emphasizes the dynamic nature of ecosystems, which is in contrast to the static assessments presented in this report.

The remainder of this paragraph raises several interesting points. Ecological integrity is linked, correctly I believe, to ecosystem processes. Yet throughout the terrestrial section of the report there appears to be a focus on static inventories of present

conditions. The models used are not dynamic and do not include representations of ecosystem processes. This shortcoming needs to be addressed if the science in the report is to be in agreement with the discussion at the start of this important section.

Section 3.1.1. “...as biodiversity and endemism increase, so does the amount of area needed...” This is hard to interpret unless it is in the context of specific biodiversity measures, and the scale of assessment is made explicit. For example, the area required for a particular level of beta diversity will depend strongly on the ecological diversity of the area. The total species richness (the measure that is often but incorrectly used as a synonym for “biodiversity”) in an area at the beta spatial scale (an area large enough to incorporate the diversity of local soil, geological, topographic and microclimatic diversity, but within one unit of macroclimatic classification) is also as much related to the ecological diversity of the area as it is to size of the area per se. Rodrigues and Gaston 2001 cited in the text here (page 108) is not in the reference list. Several examples of missing references were found (something that requires a lot of careful checking in a complex, multi reference document, but something that needs to be attended to). Considering the dramatic conclusions cited from this paper, the reader must have access to this reference.

Page 109. How was the figure of 93% of the (?) tropical area of the world “*needed to represent at least one example of each biodiversity element*” arrived at? Which “*biodiversity elements*”? Are these “elements” the same as biodiversity measures? How was the figure of 74% of the global land area needed to conserve global biodiversity arrived at? What is “*global biodiversity*” in this context? Is it the total species list (i.e. species richness)? Is it species evenness? Is it temporal biodiversity within the limits of NRV, or HRV, or some desired future range of variation (DRV)? What is “*protection*”? Is this the “*deep ecology*” interpretation of protection that would involve the essential removal of human influence from 74% of the world land area, and 93% of the tropical area? What does one do with indigenous peoples in these areas? What does one do with the still many billions of humans (although the number is declining due to the process of urbanization and migration to the cities) who live in the areas that are to be set aside? While the human depopulation of such large areas may occur over the next century, the urban population will still require resources and environmental services from the depopulated areas. How does one avoid the environmental alterations caused by 6.3 billion (today) headed for 9-11 billion within the time scale suggested on page 106, irrespective of where they are located on earth?

Do northern areas have lower biodiversity as suggested here? Or is it merely the diversity of “*charismatic megafauna*”? What about the diversity of soil insects and microbes? It may well be that the statement is true, but I am far from convinced that we are confident on this point – we simply have not yet done the work, and as biotechnology crowds out traditional taxonomy at the universities and in research, it may be a long time, if ever, before we can answer this question with confidence.

The comment in paragraph 1 that “...*site selection algorithms, by themselves, do not address the more difficult real world questions concerning the area needed to maintain viable populations of species and the persistence of biodiversity*” requires some

explanation. Are we talking here about meta-populations or local subpopulations? The issue of northern spotted owl in B.C. is often discussed as though declines or even extirpation of the very small Canadian population close to the 49th parallel political boundary threaten the continued existence of the species. I understand from such experts as Dr. Tony Sinclair of UBC Biodiversity Center that this species extends all the way down to the high elevation forests of Central America, and that from an overall species conservation strategy perspective, habitat protection in Mexico is a more important issue than local dynamics of populations at the edge of their geographic range. This does not diminish the importance of discussing conservation of this species in B.C. but requires that such discussions and the evaluation of optimal allocation of conservation effort be undertaken in the context of the full biogeographical realities of a species. So, what do “*viable populations*” of a species mean; local sub-populations, meta-populations, or the species over its entire geographical range? And how do these questions relate to the dynamics of habitat under the influence of natural disturbance and climate change?

Para. 2. What does “*the long term persistence of biodiversity*” mean? Without a definition of the measures, scales and temporal patterns of biodiversity, and of biodiversity targets, I do not know what this phrase is intended to mean. “...*maintaining the integrity of all ecosystems*” What does this mean? Does this imply the prevention of all natural disturbances and the cessation of all human impacts in all ecosystems? Does it mean maintaining ecosystems within the NRV, HRV or DRV? “...*to sustain natural ecological flows and processes on a regional scale*”. What does this mean? What is “*natural*”? Does “sustain” imply NRV, HRV, DRV? What historical period is the baseline? How do we use an historical baseline from some past climatic period in the face of the risks of human-accelerated climate change?

“*Generally, most experts have reported that some degree of protection for at least 40-60% of the terrestrial land and freshwaters would be required to sufficiently protect biodiversity.*”. Don’t we teach our undergraduate and graduate students to avoid such fuzzy statements and to make more quantitative, science-based and analytical assertions? What is the basis for “*most*”, “*some degree*”, “*at least 40-60%*”, and what is meant by “*sufficiently protect biodiversity*”? What is “*sufficiently*”? Sufficient with respect to what objectives? What measures of biodiversity? Did the analysis by Carroll et al (given as 2003, but as in press in the reference list) use process-based population models linked to dynamic ecosystem models? How did their analyses evaluate the effects of the natural fire disturbance of Yellowstone, and of fire exclusion? Unless appropriate models are used that reflect the processes involved, can the conclusions reached here be justified? The point is correctly made that spatial modeling is a *sine qua non* for all aspects of land use and resource management planning. However, as noted several time already, this must be modeling at the appropriate spatial scale using dynamic ecosystem modeling. Neither of these aspects of the terrestrial aspect of this report is satisfactory, although they do represent a starting point for such analyses.

Section 3.1.2. The major natural disturbance agents in the study area will vary according to the BEC unit involved, the location and the topography. In some areas it will be landslides, in others avalanche, in others wind, and in some areas insects. The size of the disturbance will also vary. The 30,000ha wind disturbance in the 1906 windstorm on northern Vancouver Island produced a wide variety of disturbance patch sizes in a mosaic

across the landscape. Does one relate “conservation “areas in this part of B.C. to the 30,000ha, or to the frequency distribution of disturbance patch sizes within the disturbance event area? The latter I think. The former would support the setting aside of large areas of unmanaged forest but would not reflect the local natural disturbance regime. The latter would support conservation within a managed forest using the variable retention approach. This discussion emphasizes the need for analyses that reflect the spatial scale of natural and management disturbance. The 500 ha minimal analysis scale for the terrestrial assessment does not reflect the scale of either.

Page 110. The note in para. 1 about the difficulty with the idea that “*landscapes should be in equilibrium with a disturbance regime*” is accurate, and calls for a definition in the report of “*landscape*”. Landscapes occur at many different scales – the meaning of the word is absolutely context dependent. “*Ecological landscape*” is sometimes interpreted to mean a landscape large enough that the overall character of the shifting mosaic of disturbance patches in different stages of post-disturbance development is approximately constant. In those parts of the boreal forest where fires can be up to 1 million has in size, there is no ecological landscape – the boreal in these areas is continually changing in character under the influence of climate change and very large scale disturbance. In wetter parts of the boreal where fire disturbance is smaller scale and less frequent, “ecological landscape” may exist, but periodic insect and “disease” (e.g. birch decline) outbreaks may render even these less fire prone areas subject to continual change over large areas and long time periods.

Para 2 reflects the idea of Jerry Franklin that large reserves are not the way to go in conservation in forested landscapes; rather, management of the entire landscapes as a mosaic of unmanaged reserves in a matrix of areas managed to sustain habitat elements of focal species, and target levels of OGI. This is the origin of the variable retention system that is now being widely practiced in coastal B.C. VR does not mean partial harvesting in the sense of individual tree or even group selection. Wind, diseases, soil compaction issues, safety, economic operability, roading, disturbance caused by frequent entry and other considerations do not support such partial harvesting as the best conservation and sustainable management option for the entire study area, although there may be local situations in which it is appropriate, considering the ecological diversity of the study area.

The reference to parks in other part of North America is very questionable in the context of the report. Some of the parks referred to in the context of *Island Biogeography Theory* are “*island-like within a landscape inhospitable to biodiversity and natural processes*”. While this concept has proven to have validity in oceanic islands and forest vegetation “islands” in agricultural, urban or other previously forested areas now in permanent, alternative land use and vegetation condition, it has limited validity and utility in a forested landscape such as the study area covered in this report, which is dedicated to permanent forest, parks, protected areas and forest management for a variety of values. There is considerable danger in taking research results, experiences and situations from socially, ecologically and geographically dissimilar areas and using this as the basis for analysis and conclusions about any particular area in question. This point is emphasized

in para. 3, which notes that with commitment to appropriate land use, native “biodiversity” can be sustained (which measures?) without formal “protected area” status. This conclusion seems to be at odds with the recommendations of the terrestrial section of the report that at least 40-60% of the area needs to be set aside for minimal “biodiversity” conservation. However, the analyses undertaken in the terrestrial section do not address these issues.

The comments about the grizzly bear in the last paragraph – is this based on the issues of forest fragmentation that are reported from the US? What is the relevance of this for the study area?

Page 111, para 1. The discussion of lack of knowledge and data needed to answer the questions about the needs of terrestrial focal species for permanent no-management areas is correct – we simply do not know. It also supports the need to apply the precautionary principle. However, what exactly this means requires discussion. The precautionary principle does not require that 40-60% of the entire study area immediately be put into permanent protected area status with the likelihood of future increases in this proportion, as some feel should be done – in fact this appears to be the major conclusion of the terrestrial section of the report. What it does suggest is that urgent commitments are made by government to obtain the necessary information, to use more appropriate modeling approaches, and to continue the analysis. In the meantime, forest management planning should continue ensuring that the techniques used are consistent with the habitat needs of the focal species and their protection from hunting and other human interference. The application of these techniques and the AAC control over rate of harvest means that over the next decade the area of harvesting of areas that might be of concern will be minimal, considering the size and inaccessibility of the study area. The B.C. government – on behalf of owners of the land, the people of B.C. – should commit to the collection of the necessary data using the “smart monitoring” approach developed in Saskatchewan by the Forest Impacts Monitoring Science Advisory Board. In the meantime, local human community stability should be maintained at the level decided by the LRMP process. There should be clear time lines and deliverables for a decade of monitoring to answer very specific questions, at which time the LRMP process should be revisited.

Section 3.1.4. Conclusions.

Para 1 and 2. I support the call for further studies to evaluate designs of protected areas and the data that this requires, but this must be tied to a very different level of modeling than was conducted for this report. It is a leap of faith unsupported by science from the study area to go from the correct conclusions about the need for better knowledge and models to be able to make science-based judgments about conservation strategies, to the assertion that at least 40-60% of the study area should receive “biodiversity protection”. If this is intended to mean forest management and harvesting practices that are sensitive to the habitat needs of focal species, I would wonder why the figure is not higher. If, as suggested in a media report of the conclusions of this report, this means that at least 40-60% of the study area should be put into permanent protected areas now, before we have the knowledge to know whether or not the optimum, science-

based design for such areas has been chosen, I would say that this is a value judgment and not a science-based conclusion. The precautionary principle **does not** argue for proceeding to establish even higher levels of “protection” in the admitted absence of the knowledge base from which to do this. The precautionary principle suggests a strategic, targeted research and monitoring program involving the appropriate human resources (which is not clear happened with this report) to obtain the data needed to design forest management systems and protected areas that will achieve conservation goals within a balance of social and environmental values established by democratic processes. The additional area that will be disturbed by timber harvesting in this period under the current AAC restrictions is small compared to the vast area involved in the study. It would not, as far as I am aware, threaten the key conservation issues involved. Moratoriums could be established on key areas of habitat, either in terms of limitations on harvesting techniques, patterns and roading, or in terms of temporary reserves until the fuller data base has been established.

Page 112. Again there is an inappropriate parallel drawn between Island Biogeography Theory and the realities of the time dynamic shifting mosaic of habitat conditions that is a reality in the study area. This reflects the inappropriate static analysis on which the conclusions are based. The implications of the discussion would not be inappropriate if one was comparing the progressive clearcutting of past decades (when the Ministry of Forests required the removal of all trees and snags taller than 3m, and the cutting of all non-commercial trees) with the unmanaged forest, especially if one is considering relatively short rotations of timber harvesting. However, it has much less relevance for the rates, patterns and methods of forest harvesting being conducted today at spatial scales that would have escaped detection in the spatial analysis that was conducted. In this para, there is mention of “*benchmark populations*”. I am not certain what this means? Local populations generally have gene exchange with the meta-population of which they are a component. What is the evidence of human-induced genetic isolation of populations within the study area, and what is the evidence that appropriate contemporary timber harvesting would result in such genetic isolation of local subpopulations?

Table 3.1. This table is referenced on page 111 as indicating the % of a region that should be reserved. I see no data in the table about %’s of protected areas suggested, and which biodiversity values this is to conserve. The 15 cited papers refer to the southern US states of Georgia, Florida, California (southern); Oregon and northern California coast mountains; the continental northern US Rocky Mountains (including Yellowstone National Park) and the continental Canadian Rocky Mountains; tropical rain forests, Australia, Californian islands, and some general estimates based on a review of 21 worldwide studies. Without a discussion of the ecological and land use situations and the ecology and biology of the biodiversity values involved in these generally different areas, the reader is at a loss to know how these studies lead to the conclusion of “at least 40-60%” protected area requirement for the study area. There is no mention in this table of a study in the ecological and geographical area of the study. Certainly, in the absence of the desired local knowledge, experience and research from other areas is a valid alternative, but only if the relevance of the knowledge and experience from these other areas for the study area is carefully assessed and reported.

Section 4. Human Impacts.

Page 119. para 1. “*Biological integrity of terrestrial ecosystems*”. What is meant here by “*biological integrity*”? It is not clear. Maintaining a constant species list; maintaining constant populations of organisms? Neither of these is possible, natural or an appropriate conservation goal. Why does such integrity depend largely on “*previous human alterations*”? Without knowing what is meant by biological integrity, it is hard to know whether this depends on human impacts.

The statement that “*ecological systems are adversely affected by human alterations*” is scientifically untenable without definitions, although there are certainly innumerable examples of ecological systems at various scales of space and complexity being altered by both natural and human interactions. What is meant by “*adversely*”. Ecology is the science of the study of ecosystems. It is, or should be, value-free. It should describe, explain and predict, and leave it up to social (including philosophical) systems to make value judgments. There are many examples in which human-caused disturbance has raised the level of many measures of biological diversity from that in systems not so disturbed; where such disturbance has increased energy flows, nutrient cycling and biomass. There are many examples in which human disturbance has had the opposite effect. The statement as presented is far too general and should be considered a value judgment, not a generally valid, scientific, cause-effect relationship.

I agree with the rest of this paragraph. No simple, universal relationships have yet been established between ecosystem function, measures of diversity and stability, and various human-caused and non-human disturbances. Such relationships may exist in certain limited circumstances, and for those circumstances they may be predictive and a suitable foundation for predictive models. However, the transferability of such relationships has generally proven to be difficult between ecologically different circumstances. Because there is as yet no widely accepted definition of “*ecological integrity*”, it is not possible to discuss relationships between this and other terms. The lack of an agreed-on definition, and the lack of a definition of how this term is being used in this report makes the statement in para 2 page 120 problematical.

Section 4.2. Methods.

Page 120. What is the basis for the 200m road buffer? Is this 200m each side of a road, or 100m each side of a road?. Is the road effect permanent? If the road is through a harvested area, as the trees grow up the road effect will diminish, depending on the width of the right-of-way, until many aspects of the road effect will disappear as the tree canopies close over the road. Is the effect permanent even if the road is closed to traffic? Is it permanent if the road is de-activated? It would be helpful to an evaluation of the methodology if these questions were clarified.

Earlier in the report (page 20) I gained the impression that watersheds with >2% human caused disturbance were permanently “developed”. Table 4.2 introduces a second level of classification – “intact”, “modified” and “developed” in which “developed” means “ >25% area impacted (what does “*impacted*” mean – is it a permanent condition, and does it depend on silvicultural system, method of harvesting, degree of soil disturbance, etc) or >0.6/km road density (does this refer to skid roads, secondary roads, main haul roads, temporary roads, rebuilt roads?)”. In neither case is the time dimension addressed, nor the benefits as well as costs of roads for various different species. Table 4.2 does not provide information on how the distance of disturbance to rivers and streams is handled, how this relates to stream order, stream channel character, fish/non-fish bearing (stream fish habitat classification), slope, geology, soils, climate, slope stability and vegetation cover factors. All of these will influence how distance of disturbance to streams/rivers affects the character and habitat value of the stream section in question and downstream reaches.

Page 121. Table 4.1. The title of this table does not reflect its contents. Some of the citations do not refer to measures of biodiversity. Is the relationship attributed to Mace et al 1996 a causal one? Do female grizzlies actively avoid areas with > 0.6 km/1 km², or have roads been restricted in bear habitat, or is such habitat located in areas where roads would not normally be built for other reasons? Similarly for Findlay and Houlihan 1997. Roads in wetlands are generally built on drier, higher microsites which naturally have different vegetation and different measures of biological diversity. Which measures of biological diversity were they studying, and did they account for road location-site effects? The assertion of 1-2 km effects suggests that this is a road and not site effect, but site could be a confounding factor.

Jones et al studies – what type of watershed, what watershed order, what type of climate, snowpack or rain-dominated hydrograph? In a watershed in the southern interior of BC, there was no significant difference reported in hydrograph and regimen in a recent (unpublished) study from when <10% of the approximately 20,000 ha watershed had been roaded and harvested to when virtually all the watershed was roaded and much of it was harvested and in young second growth– the result of salvage harvesting of Mountain Pine Beetle outbreak. Clearly, very divergent results can be expected in different ecological settings. Different landscape patterns of harvesting can also have different hydrological effects, especially if different parts of the watershed have different snowpack conditions and climates. The soil contamination references; contamination by what - oil, road dust, salt, other? The references cited are not in the reference list. What “thresholds” are being referred to? Elk and other ungulate avoidance of roads - during active logging? What happens if the roads are closed? Is this an effect of the roads themselves or of hunting from the roads? If the effect is hunting/human access and mechanical activity, how would concentration of harvesting that minimizes road density and length, minimizing the frequency of mechanical activity, and closing the road after active use affect the noted avoidance reaction? Forman and other citations – what is the effect of harvesting aside from roads? If helicopter or long line harvesting is done, what is the effect?

Table 4.2. Is an area “intact” if it has been naturally disturbed? Wind-thrown, root rots, insects, landslides, fire? Mount St Hellens? How does this compare with variable retention or partial harvesting with no roads – yes, it takes the wood away but so do some natural disturbances. Please define “impacted”, as noted earlier.

Table 4.3. Why are the labels given here used rather than stream/watershed order? More understandable by the lay reader perhaps?

Section 5. Spatial analysis: Methods.

It would be helpful to have a discussion of the spatial analysis earlier in the report since so much is based on it.

Page 124, line 1-3. Right on – this type of analysis requires information on the future condition of the landscape and specific stands. I concur that this report must be regarded as a first step. The Team is clear and scientifically credible on this point. The next step is to repeat this useful first step with more appropriate modeling and an intensive information gathering/monitoring (using the “Smart monitoring” approach”) program. There needs to be a very different minimum spatial scale for the terrestrial analysis, it must be driven by dynamic, process-based ecosystem management models, and there needs to be much more analysis of the sensitivity of conclusions to species penalties (which should be species and site-specific) and boundary length (which should consider the temporal dynamics of the ecological character of boundaries, and the effect of variable retention systems, cutblock size and shape, edge feathering and sinuosity/softness of boundaries on boundary length penalties).

Page 125, para 1. “*Local scale imperiled species*”? Does this refer to risks of local extirpation? What is the role of such local extirpation in the stability of meta-populations, and predator-prey and disease-prey relationships? What are “*connectivity*” and “*fragmentation*” in a dynamic, shifting landscape mosaic of non-declining local (stand-level) change, especially if the forest is managed by a VR system, subject to concerns about tree species diversity (the light and soil disturbance needs for native - the only ones used in the study area - deciduous and coniferous species that require significant openings for their natural regeneration and the growth and survival of planted and naturally regenerated species).

Para 4. Marxan studies – if there is no single “correct” run (I agree with this), how do you design the system? How do you know that the average of many runs is the “best design”. Does such an average reflect the mathematics of the algorithm or the biology/ecology of the conservation values of interest?. This will depend in part on the degree to which the focal species sub-models have incorporated causal relationships rather than simple correlations.

Page 126, Para. 1. What is the empirical evidence that a few larger reserves are better than a larger number of smaller reserves in a shifting mosaic of managed stands with VR? Did the terrestrial analysis examine the concept of shifting reserves such as has been

used in Tasmanian temperate rain forests? The penalty of boundary length appears to be based on a static view of forest edges, which is not reasonable.

Para. 2. Perhaps the single greatest flaw in the study is the use of the 500 ha planning unit. What scientific basis is there for choosing this scale in part because the Marine study used this? Where in the report is it necessary that such a similarity exist? The choice of a fixed size in a system in which biological diversity relates to many different scales, many of them much smaller than 500 ha, is not a reasonable basis for a biodiversity measures conservation plan in this area of steep environmental gradients and relatively high beta diversity.

Para. 4. What is a “viable *example of an ecological system*”? Does the report mean areas of high OGI? But high OGI stands can be produced by VR systems. Management can increase the OGI of old forests with relatively low OGI.

Clearcuts are no more comparable to urban and agricultural areas than most recent clearcuts are comparable to some types of recently naturally disturbed areas. To have lumped these in an index of suitability seems to reflect a profound lack of understanding of the character of forest ecosystems, their dynamics, processes and ecological succession pathways. Alternatively, perhaps this merely reflects, accurately, the level of modeling used, rather than the knowledge of the modelers. If the latter is the case, why was their knowledge not incorporated into a more ecologically-credible analysis using more ecosystem-based models?

Given my concerns about the approach used, I will not critique the details of the algorithms. Because I do not accept the scale and the static character of the analysis as being appropriate, the details of the technique are not relevant to this review.

Page 127. Para. 1. Considering the wealth of experience in the Team, I am surprised that they were not able to use different weightings for the species penalty factors. In the absence of different weightings, there should be a sensitivity analysis of the implications of using the same factor weighting for all species.

Para. 4. Perhaps I missed it but I got to this section without understanding exactly what is meant by the conservation goal levels. This is supposed to be explained in Section 2, but it is not. My lack of understanding of exactly what these goals are is a problem when I come to Section 6.

Page 139. Para. 1. Table 6.2 does not describe the six impact classes as suggested in the text.

Table 5.2. If an ecosystem has a low conservation values, why is it classified as a 2 when it is “intact”. If “intact” confers conservation value, then it would be medium to high conservation value. What is the logic here?

Section 5.4.5. Is there more than one Central Coast LRMP? What “tables” are referred to here? I thought that there was a single LRMP table with multiple stakeholders. Perhaps each separate stakeholder group is considered as a table. It would be helpful to explain this.

6.0 Spatial Analysis: Results.

Why are Terrestrial and Freshwater analyses lumped? What would the results have been if they were analyzed separately? I thought that Freshwater used a different spatial scale than the terrestrial, but on checking the Methods section I could not find a description of how the Freshwater spatial analysis was done? Did I miss it or does the report need to explain this more clearly?

Table 6.1. Does the analysis incorporate the non-contributing land base? What exactly is the meaning of “identified goal thresholds” This is not adequately explained.

Figure 6.2. Why does the line for 100% goal threshold reach less than 18% when all the presently protected areas are locked in? Does this suggest that they were badly designed for conservation? Should they be scrapped and a new protected areas plan devised? Does this suggest caution against rushing into a new reserve design considering all the caveats in the report about lack of data and knowledge, the inadequacy of many focal species models, and my concerns about the whole SITES modeling exercise?

Page 145. Para. 1-2. These paragraphs are difficult to interpret. Are they saying that the size of the study area is too large to reach meaningful conclusions? I agree that the spatial scale of SITES prevents meaningful conservation conclusions from being reached. The last part of para. 1 is hard to follow. It needs to be simplified. I do not understand what is being said in para. 2.

Section 6.3

I found this to be better written and easier to follow than the Terrestrial /Freshwater section.

Section 7

Para.1. From my knowledge of the present structure of QUEST, and the design, time frame and level of detail in this useful heuristic rather than predictive planning tool, I cannot see the rationale for planning to use it as the basis for future studies. There are many much more appropriate planning tools available that can capture the spatial scales, the ecosystems processes, and the temporal dynamics that are so critical for conservation analysis. I have been involved in the forestry part of Georgia Basin QUEST, and in several other modeling tools, so I make this judgment from the basis of some knowledge of the field. This takes nothing away from QUEST as a tool to achieve the objectives for which it was designed. It simply says that there are better and much more appropriate tools available for the purposes of the Central and North Coast LRMP processes. QUEST does not have the spatial and temporal resolution needed for this type of work.

The question must be raised, why were these other tools not considered as part of the present analysis, and why were they not proposed for the necessary continuation of the

modeling work? Was this a deliberate choice, or simply lack of knowledge of these other tools on the part of the Team?

8.0 Conclusions.

This section of the report was missing from the copy I was asked to review.

4. Final Comments and Recommendations

The issues addressed in this report are important – much too important to be done hurriedly and in error. It is my opinion that the task assigned to the ESA Team was unreasonably large for the time and resources that were available and/or used. It also seems that for undeclared reasons the Team chose not to avail itself of abundant talent within the province – in the Ministry of Forests, the universities, the consulting community and the licensees. My major conclusion is that this report should be tabled as a useful first step, but that the government, on behalf of the owners of the resources – all the people of B.C. - should undertake to fund a further and much more detailed analysis using much more suitable tools, and engaging all the local experts. These comments are directed mainly at the Terrestrial section of the report. Others in the freshwater and marine community of experts should comment on whether the same is true for those other sections.

The major recommendations, other than the numerous editorial comments, questions and suggestions I have made, are that the terrestrial analysis be redone using a spatial resolution that is more appropriate for the landscape and values involved. It should be based on ecosystem management models at the stand level driving landscape level model(s) that operate at much smaller spatial scales while still addressing the very large area involved in the study. This calls for cross scale modeling tools of the type that are currently available and being used in the province. It is unclear why these were not used. The predictions of these or any other models should be field verified to the extent that this is possible, and the LRMP recipients of the report should be taken on appropriate field trips to consider, on the ground and supported by local experts and resource scientists and managers, the efficacy of the modeling and its ability to address conservation issues. Hi-tech presentations of modeling output in the absence of field “trudging” can easily lead to erroneous conclusions and actions. The terrestrial work should be tied closely to the BEC system and should include the MoF, university, industry and consultant personnel who are expert in this system and the ecology of the coast, but who were not involved, or not adequately involved, in this study and report so far. Input from local First Nations who have occupied the region for many generations should also be a key part of the re-analysis

I realize that the ESA report will be used as input to the LRMP process and that this is time-limited. I suggest that its use in this context be restricted to the status of a progress

report and a useful statement of the opinion of the Team. It represents a unique assemblage of knowledge and data, albeit inadequate for the enormous task they were given. Its value in this context should be recognized – it is a useful first step, but only that. There are too many serious concerns about the methodology, the quality and extent of the data and the strengths of the tools used – all freely pointed out by the Team in the report – for it to be used as a basis for far reaching decisions about land use in this area. The report as it stands certainly informs the LRMP about the nature of conservation issues in the study area, and what we do and do not know about these issues today. But by the admission of the Team, the existing data, knowledge and models are not adequate to arrive at quantitative decisions with respect to the important conservation dimensions of land use planning in the study area.

If this report is to be considered science-based, the treatment of ecological terms and concepts needs significant work. In particular an error that has plagued our ecological science must be avoided – that of generalizing useful concepts, models and data to beyond the domain in which they have been tested and/or shown to be valid. They should not be used in different environments for which no such test exists or where it has been shown, or local knowledge and experience indicates, that they are inappropriate. There must be a clear distinction between belief systems and mythologies about conservation issues, and the science-based testing of those belief systems and mythologies based on rigorous scientific methods.

As Aldo Leopold said:

“The evolution of a land ethic is an intellectual as well as emotional process. Conservation is paved with good intentions which prove to be futile, or even dangerous, because they are devoid of critical understanding either of the land, or of economic land use.”

J. P. (Hamish) Kimmins
January 12th, 2004

