

March 29, 2004.
CIT Secretariat

Re Author's Response to Central Coast ETRA Peer Review.

Here is our response to the review of our report by Dr. J. Anderson. We found the review to be very thoughtful, insightful, and thorough.

Due to the lengthy and detailed nature of the review, we respond here with our general comments only. Most of the reviewer's recommended revisions specified in category two below, have already been addressed in our revised report.

The reviewer's comments fall into two main categories:

1. Comments about the meaning and presentation of key terms such as "risk". We provide our response below. We did not change the entire text to accommodate the points raised, but did provide clarity early on in the document. See discussion below.
2. Specific comments where suggestions to improve readability or correct errors are made. We incorporated responses/ clarifications to all these types of suggestions into the revised version of the ETRA, and do not identify each particular item here.

Interpretation and Presentation of Risk

A primary thrust of the reviewer's comments stems from our use of the word 'risk'. In general usage the concept of "risk" combines 'significance' with 'probability' (as in "likelihood of an undesirable outcome occurring"). Our interpretation (though not one shared by the reviewer) is that we actually use both terms. 'Probability' is explicit and is intended to be interpreted as 'the probability that present ecological integrity provided by the coarse filter/ old forest will be maintained'. *Implicit* in the term is the assumption agreed to by the ERA team that the "loss of ecological integrity is undesirable". We do not deal with 'degrees' of loss of integrity because the data / knowledge do not allow us to do so. The changing variable therefore is only probability of loss.

The reviewer states that the ETRA (Holt and Sutherland 2003b) defines the risk of interest as "that the coarse filter functions will be less likely to be maintained, and that species/processes/ecosystems will eventually be lost or degraded." In fact the definition says:

'This 'coarse filter' risk assessment focuses on the risk / **probability** of failing to maintain an adequate coarse filter management regime which we assume to be related to the probability of maintaining ecological integrity. '

Failure to maintain ecological integrity is therefore hypothesized to result in species / ecosystem extirpations from areas of the coast. In addition, the complex and largely undocumented processes that maintain the ecosystem will be disturbed and ecosystems will not provide their natural services. The 'ecosystem integrity' and its loss is the outcome the analysis intends to focus on. We do not explicitly state the idea that some people may not identify this as 'bad'. However, the coastal LRMP processes are working on the mandate of ecosystem-based management which explicitly identifies a goal of maintaining integrity.

The reviewer suggests that we focus on an explicit, measurable change in a variable, rather than a less explicit measure such as "risk". For example, the reviewer states: "as an indicator of the undesirable outcome we're concerned about, OFDiff seems reasonable, and I would rather see the quantity being analyzed referred to consistently

as 'OFDiff' or some other empirically descriptive name, rather than label it "risk" in text and tables."

We agree that this may have been a better approach in retrospect, but would require substantive change in the text with no major benefit.

The reviewer suggests that there are other disadvantages to renaming OFDiff as "risk". For example, OFDiff is already a highly derived, simulated variable. We disagree in part with this - OFDIFF is a pretty straightforward variable, given the data we had to work with. The variable does include various uncertainties originating with estimates of mean RONV in the different coastal ecosystems, in our ability to accurately 'age' from inventory methods, and in the projection of stand age through time. More problematic is deciding whether "risk" can be interpreted the same way as you diverge from OFDiff in opposite directions. We aren't reporting about functional relationships – just 'probability that functioning is lost, or perhaps more specifically "changed" from the natural condition now observed. It is not easy to work out how to simply describe effects on ecosystem change when some species may benefit and others may not.

We did clarify the assumptions in the text early on in the document, and state very explicitly that the value being measured is 'difference in old forest from natural', but that it is being categorised in a one-to-one fashion into risk categories. The reader can use their own preference to interpret the outcomes, if they do not agree with the use of risk.

Verbal categories of risk

The reviewer is concerned about our use of words like 'cautious in Figure captions and text. The category boundaries are not really "cautious" themselves, nor should moving them in one direction or the other imply a cautious policy decision.

We have remedied this by changing the text.

Tricky variables

The reviewer is correct in noting that the variables used for analysis contain several difficulties. One of these is interpreting ratio variables that have complex error structures. We agree with these comments, and temper the worry slightly by stating that: in all our analyses the denominator stayed the same, thus removing part of the potential problems in comparing and interpreting them.

The Bayesian Belief Network

In general, the reviewer asks for more clarification on how the BBN works, and is used in the evaluation process. More specifically, we summarize the questions as: (1) why choose a framework based on a Bayesian methodology over some other possible methodology, given that different methodologies have strengths and weaknesses for a particular type of problem; (2) how were some of the conclusions arrived at using the BBN; (3) how was uncertainty dealt with in the BBN; and (4) are the conclusions reported on stated appropriately, given the types of hypotheses considered, the structure of the input data, and the Bayesian form of the BBN analysis?

A thread running through these review comments was the desire to see a complete worked example, from the model outputs (the Woodstock model, not SELES in this case as inferred by the reviewer) through the network, to the two types of conclusions, base risk and risk category. This is certainly possible to do – we did not

have budget time in our respective contracts with CIT to easily reassign task effort from doing the assessment to explicating the methodology. Nor is it clear how much that would help the end users of the results – the LRMP Table, which was our target audience – discussing probabilities, and terms like BBN is difficult even amongst academics, and is even more so among people who have to make decisions. Certainly a worked example would likely clear up many of the questions raised by the reviewer.

Specific comments from paraphrased list above:

1. Is the Bayesian methodology (as represented by the BBN) suitable in this situation? We chose the BBN framework as much for its practical utility in representing the different hypotheses about factors that could contribute to deviations from ecological condition, and the probabilities that link them (given model results and uncertainties), than whether or not it was a “Bayesian” approach or not.
In fact, we used the Netica BBN solely as a conditional probability Markov-chain network, where the nodes represent variables (states) of interest (e.g. deviation from RONV) and the connections between them represent probabilistic dependencies. The nodes themselves were represented generally as discrete states (base risk is an example: 0-20; 21-40, ...; 81-100), with probabilities of the values of each category being “true” conditioned on the probabilistic states of all the other nodes in the network.
The use of Bayes’ theorem in Netica is limited to the way the BBN framework uses to update its weighted strength of belief in the nodes (states) having a particular value with new information. What we actually did when reading in new model results, was to set the weight of the prior states of the network to a very low ($1e-6$) value relative to the new information (the new model data). So, we did not depend on or use the “Bayesian” part of the Netica BBN in our analysis.
In designing the approach, and choosing whether or not to use a conditional probability method, like the BBN, we thought that there could be a wide set of hypotheses that could influence conclusions about future ecosystem state. That the BBN approach seemed a good way to represent these. In fact, in the case of the analysis of the CC model results, there were very few factors we could actually consider in our assessment, and so the power of the BBN to do such things as test the sensitivity of conclusions to different hypotheses about the contributions to ecosystem state was not used in our analysis. Other possibilities, such as Monte Carlo analysis or Scenario analysis (to mention 2 suggested by the reviewer) would work too – the latter was in fact done in combination with BBN in the North Coast analysis.
2. The reviewer specifically asks how the risk category conclusion is drawn from the base risk number in the BBN? The answer is that the “Base Risk “ value is an expected value (a weighted mean value), while the final risk category is the “most probable” value – the modal category. In cases where there is wide distribution of probabilities about the “base risk” state of a given ecosystem in the landscape, the most probable value may indeed be different than the expected value. Indeed, considerable information is lost when expressing risk as a single risk category rather than as a distribution of possible values – the authors were concerned about this, but were not sure how to capture simply the essence of the conclusion from the point of view of decision-making.
3. Uncertainty is carried through the network in a straightforward way, because the conditional probabilities multiply together (remember it is a Markov-chain belief network) as specified by the linkages between nodes. The keys are: what type of

uncertainty, and where is it represented? Uncertainty in the states of each variable (node) is represented by the probability of each state being true, given the inputs to that node. We tried to represent the state of scientific knowledge about what different input values would imply about the likely state of a node value – where scientific data existed to guide us. In the case of coarse-filter biodiversity, there often isn't much data, and so we tried to represent expert opinion as best we could. A useful extension of this approach (beyond the scope of this project) would be to more exhaustively examine the influence of different assumptions about these probabilities on the conclusions.

There is another type of uncertainty – uncertainty about the future state of the forest. This uncertainty is represented by the different model scenarios, and their resulting inputs to the BBN. In this analysis we were only able to analyze one scenario (the Base Case), and thus cannot directly assess this. That can only be done by a comparative scenario analysis.

The reviewer makes reference to “point estimates” of outcomes. Yes, this loses some information. If probability distributions can effectively communicate results to Table members, then they should be used. Our experience has been that people don't know how to think about probability distributions without a lot of guidance.

4. I think point 4 has been addressed by our responses to the previous points. The challenge is to communicate relative probabilities to decision-makers.

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