The Cryosphere Discuss., 6, C3063–C3067, 2013
www.the-cryosphere-discuss.net/6/C3063/2013/

The Cryosphere Discussions

Interactive comment on "Scenario-based climate change modelling for a regional permafrost probability model of the southern Yukon and northern British Columbia, Canada" *by* P. P. Bonnaventure and A. G. Lewkowicz

P. P. Bonnaventure and A. G. Lewkowicz

philip.bonnaventure@queensu.ca

Received and published: 26 February 2013

Authors' response to reviewers' comments on "Scenario-based climate change modeling for a regional permafrost probability model of the southern Yukon and northern British Columbia, Canada"

We are grateful to the reviewers for their thoughtful comments. We agree with most of them and have incorporated the changes into the manuscript. We have dealt with the comments one by one and explain the changes that have been made to the pa-

per in response, or in a few cases, why we feel that the suggested changes are not appropriate.

Anonymous referee #1 We agree that the title of the paper is not as clear as we had planned. The title has been changed to "Impacts of scenario-based climate change on a regional permafrost probability model for the southern Yukon and northern British Columbia, Canada". We agree that modeling was needed for the intermediate (but probable) levels of warming by +3 K and +4 K. These were undertaken and the results are now included in the abstract and the text as requested. We agree that the assumption of a constant -6.5 K km-1 above treeline is a limitation of the model. Unfortunately our monitoring sites for this lapse rate are too few in number to apply a spatially variable rate. However, three sites that cover the range of conditions suggest that a uniform rate above treeline is reasonable. At Haines Summit, the most maritime measurement site, the rate is -6.4 K km-1 above treeline. At Wolf Creek in the southern Yukon, the SLR is -6.1 K km-1 above treeline. At Dawson, the most continental site, the rate above treeline is -6.8 K km-1. We have added several sentences to indicate why we believe the assumption to be justified and its relatively low impact on the results. The "present-day" is 1971-2000 because one step in the modeling required us to use the climate normals for the Environment Canada stations for this period. This point has now been included in the abstract and in the text. Page 4519, lines 20-27. We agree with the referee that precipitation and hydrological changes should not have been included in this list which applied to "permafrost". We have changed the start of the sentence to "permafrost regions", the reference to "precipitation" has been eliminated and the hydrological change has been specified as "runoff changes". Page 4523, lines 10-15. We agree that the use of the term validation when comparing one model to another is incorrect. We have added more details to the text to summarize better what was done in Bonnaventure et al. (2012) to test the predictions. Comments on scale, confidence and use of the model (page 4524, line 3 and page 4534 lines 1-5). Our text apparently does not differentiate sufficiently between uncertainties in the degree of climate change and uncertainties in their impacts. The comments on page 4524 refer to the

climatic scenarios while those in the conclusions refer to the impacts on permafrost probability. We do not have well-constrained indications on how much cooler it was in the Yukon during the LIA (page 4524 lines 1-3) and the GCM/RCM future predictions for the region are also uncertain given the topographic influences (page 4523 lines 20-23). Because we cannot be certain about the degree or timing of past or future change we used scenario-based modeling. On the other hand, our modeling indicates the patterns of change that can be expected under these climatic warming/cooling scenarios and we are confident about the impacts for a given degree of change, providing the time is available for the ground to equilibrate with the climate. Resource infrastructure project proponents will have to select and justify what degree of climate change they are planning for in their environmental impact assessments. Our modeling will then show what can be expected in terms of change in permafrost probability. We have changed the text on page 4534 to better reflect the goals of the modeling. Page 4532 line 15. Added "in the forest" to clarify and at other places in the text. Comments on snow. A new short paragraph has been added at the end of the model limitations mentioning snow. Page 4532 lines 26-29. Sentence has been changed and the entire section has been shortened. Figure 1. Changes made in the caption to identify the location of Pleasant Camp and the meaning of "modeling locations". Reference list now includes Bonnaventure et al. (2012) and DOIs where available. All technical corrections made.

Anonymous referee #2 General concerns about reference to previously published works. We thank the referee for the comments. We certainly want the paper to be clear to readers. We have added information at numerous points in the text in order to clarify the modeling procedures. However, we could not add all the information in the previously published works as this would have created an impossibly long manuscript and we do not feel it is appropriate that each time we publish on the use of the regional model that we must repeat its derivation in full. The previously published paper that covers the development of the regional model is Bonnaventure et al. (2012) and we have now indicated this so that a reader can obtain full information

there. Specific comments 1. A) DEM resolution of 30 x 30 m is now mentioned. B) BTS measurements could not be made in all parts of the landscape as snow depths were too low at many sites. However, BTS measurements made at specific locations could be modeled across the entire area. The BTS model field was then calibrated using direct observation of the presence or absence permafrost in summer to produce a logistic regression model. The modeled probabilities are therefore for "typical" snow depths since the summer observations were made randomly. Earlier work (Lewkowicz and Ednie, 2004) showed that permafrost probability can vary over short distances in relation to snow depths in critical elevation bands. The present modeling is for typical conditions and does not take this local-scale variability into account. An additional sentence has been placed in methods to make this point clear. 2. Units. We have used K in a previous publication involving climate change modeling (Bonnaventure and Lewkowicz, 2010) and prefer to be consistent. The referee is correct about lapse rates so we have changed the units to K km-1. 3. The order of Figures has been corrected. 4. Table 1 has been added to summarize the results. Additional text has been added in the results section to describe and bring out the main points. Nothwithstanding these additions in Results, we have edited the paper thoroughly so that the main text is actually 15% shorter than the original submission. 5. +3 and +4 K scenarios were run and the results are now presented. 6. We respectfully disagree with the reviewer's recommendation to use a single colour and a 5-category legend. We have used an 11 class colour scale in several previous publications and believe that red to blue is an effective means to show permafrost. Moving to a 5-class scale would exclude a great deal of the detail in the modeling results. 7. The modelling results are at a 30 x 30 m resolution. We have generalized the patterns by upscaling across the region in Figure 15, but the actual model is at the hillslope scale, as shown in (now) Figure 12. We regret omitting the scale of the model in the original text which would have clarified this point. In terms of the SLR values, these were derived from field measurements. There was a strong correlation between these measured SLR values and the degree of continentality as expressed

by the difference between the mean January and mean July air temperatures. This allowed us to develop a trend surface for equivalent elevation. The derivation of the equivalent elevation trend surface is fully described in Lewkowicz and Bonnaventure (2011) and Bonnaventure et al. (2012), but we have also added text in the Methods section. Technical issues Corrections have been made and a thorough edit carried out.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C3063/2013/tcd-6-C3063-2013-supplement.pdf

---

Interactive comment on The Cryosphere Discuss., 6, 4517, 2012.