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Interactive comment on "Mapping snow depth within a tundra ecosystem using multiscale observations and Bayesian methods" *by* Haruko M. Wainwright et al.

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Review of "Mapping snow depth within a tundra ecosystem using multiscale observations and Bayesian methods" by H. M. Wainwright

Thanks you very much for asking me to review the paper.

The paper addresses the question of mapping snow depths over large area, taking into account various data of different resolutions and precisions, in order to obtain the best possible interpolated and area wide estimates. The study area in a tundra ecosystem, in north Alaska. The topic of the article is clearly of high scientific interest, especially in the chosen study context, and fully in the scope of The Cryosphere, being poten-

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tially of high interest for a large part of its readership. The amount and variety of data brought together and the amount of work carried out to analyze them and reach substantial conclusions is impressive. I am not a specialist of snow depth variability mapping and modelling, so I am not fully able to judge exactly to which extent the author's approach is completely novel or not, but the author's novelty claims seem fair (they use well known methods at several steps of their workflow, with adequate references). Especially, the use of a Bayesian approach to combine the different multiscale datasets available seems to be innovative in this field and very well designed to reach the objective of the work. Also, its straightforward potential extension to space-time data is indeed promising. Formally, the paper if written in an excellent and concise English, and well organized. Tables and captions are of sufficient number, with adequate captions. All in all, I therefore largely support the publication of the paper in TC, after taking into account the comments below:

Workflow: Because of the several modelling steps and datasets, following the workflow is not that easy. An additional figure would really help.

Spatial modelling: Maybe it is because I cannot fully figure out the workflow, but the way the explicit spatial modelling is done is not crystal clear to me. As far as I understand, three random fields are fitted on the data (presumably using max likelihood techniques), and the corresponding variograms are used as known covariance matrixes in the Bayesian modelling step. If this is true, the claim of a Bayesian geostatistical estimation method is not justified, or at least not fully. I feel I am right because the conditioning on tau appears nowhere in the detailed conditional distributions. Also, it is clear that inferring the variograms adds substantial difficulty to the Gibbs sampling. Yet, it remains feasible, see Lavigne et al. (2016) as an example. Anyhow, things should be written more clearly and the choice made justified.

Modelling assumptions: The model proposed is rather straightforward, and, in fine, seems to be sufficient in cross-validation. However as the paper is written, the modelling choices made seem to be a bit too much convenience choices for the Gibbs

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sampling rather than data-driven, letting the reader the feeling that alternative modelling structures could do even better. We indeed all love handling only Gaussian fields, but sometimes things are a bit more complicated. For instance, I would expect some skewness in the variables analysed by the authors. Also, there is no justification for the three chosen variogram models, etc. Even if no sensitivity study is attempted (the paper is already long), a few words should at least be added in discussion to justify the probabilistic choices made.

Reference: Lavigne, A., Eckert, N., Bel, L., Parent, E. (2015). Modelling the spatio-temporal repartition of right-truncated data : application to avalanche runout altitudes in Haute-Savoie. Stochastic Environmental Research and Risk Assessment. In press.

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