

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-341

Anonymous Referee #1

Referee comment on "Spatial patterns of snow distribution in the sub-Arctic" by Katrina E. Bennett et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-341-RC1>, 2021

General Comments:

Bennett et al., perform a detailed experiment at a set of study sites in Alaska to answer the increasingly important question of "how much snow exists here". Their work examines a suite of regression models of varying sophistication to model SWE based on a set of environmental predictors like NDVI, elevation and wind. The paper was detailed, well written with a novel methodology and promising resulting model performance from the RF (suggesting followup work in this area of research). While portions of the paper are a bit verbose, after some edits I believe that this paper would be an important scientific contribution for the readers of The Cryosphere.

Major Comments/Revisions:

1. While the paper by Bennet et al., is generally well written, it can be overly detailed in certain places. With some restructuring, I believe the paper can be much more concise and effective. For instance, the Introduction from lines 50-95 is likely unnecessary content. I would much rather get right into the meat of the problem at hand starting on line 96, as information about the importance of snow etc. can probably be a sentence or two with details left in references to previous literature. I have similar comments for Section 4 (specifically 4.1, 4.2, 4.3 and 4.4) which should not be in the results section and likely could be summarized in either the methodology or introduction in a paragraph or two. The beginning of 4.4 was answering questions I had about the model setup described in the methodology. These should absolutely be grouped together and the structure revised for clarity. Finally on this point, the discussion section 5 is again far too verbose and should likely be restructured with some of the details moved to the results section or moved to the Appendix. I would recommend limiting the discussion to a summary of uncertainties, sources of error and questions left unanswered from the results.

2. Line 462, I am interested/potentially concerned about the extreme importance of Year on the accuracy of your RF. The authors mention that this Year variable is in some ways a proxy for temperature/precipitation differences between years, and I would ask why not explicitly test for this? While I agree that this conclusion is probably correct, incorporating temperature and precipitation data from a well-validated reanalysis product like ERA5 or MERRA2 could help evaluate this hypothesis. It would also help explain which of these two variables is the most important. Furthermore, a Year variable really limits the robustness of this product for applications outside of your current study and removing it would help in predictions elsewhere. For instance, what if you want to apply your model to data retrieved last year? Would the RF understand the year value of 2020 if fed into the model? However, it could, in theory, incorporate precipitation/temperature data from 2020 without issue.

Minor Comments/Revisions:

3. Can you speak to the different sampling distributions in Fig. 2? While I realize the Kougarkok site is much larger than Teller, the spatial coverage of the samples display much more structure and consistency at Teller than Kougarkok and I am curious if this sampling discrepancy impacts your results and why the sampling was so different.

4. Regarding model training I had a few questions. First, what sort of hyperparameterization are you using? It appears to be a RandomSearchCV but this isn't explicitly mentioned. Why not use something a little more sophisticated like a Bayesian Search? Furthermore, why do you use an 80/20 split for train/test instead of a kFold CV like you do in the hyperparameterization step? This way you can operate on the full dataset.

5. A table in the appendix showing the different final models (like the RF) and all incorporated predictors would be helpful for clarity.

6. Regarding the title, is this truly an Arctic analysis? The sites are all straddling the Arctic circle and some may consider this to be in the sub-arctic region.

7. I commend the authors for speaking to the topic of complex terrain in different areas of the manuscript, however I am curious how your model accuracy would change as a function of the complexity of the terrain in mountainous regions. For instance, could this be applied to an alpine area? These regions don't typically have much plant life so I would expect a predictor like NDVI would be much less useful here and furthermore, the

distribution of snow is extremely heterogeneous across these locations.

8. I am curious why you selected the RF over a method like a neural network? With such a large sample, I would expect a deep learning method like a multilayer perceptron would perform as well or better than the RF. This may be outside the scope of your paper, but something to consider.

9. Fig. 3 caption should not have the definition of SWE in it

10. Section 4.2 heading isn't capitalized while the same words in 5.2 are? Just wondering for consistency.