

Interactive comment on “Real-Time Snow Depth Estimation and Historical Data Reconstruction Over China Based on a Random Forest Machine Learning Approach” by Jianwei Yang et al.

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The manuscript aims to reconstruct the historical snow data set and to develop a real time snow depth estimation. I qualify this manuscript somewhere between major revision and rejection. The major revision is because the MS has some serious issues in methods, validation and some of the statements are not supported by the result. On the other hand the historical snow data set is an interesting product (if properly validated). The rejection is due to lack of novelty in this study: Authors use well established methods in a standard way and what they obtain is a product that has a similar RMSE as a former product available for China.

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Major issues:

I agree with the Anonymous Reviewer (point 9b), who pointed that this complicated methodology of using RF to produce more data is probably unnecessary and that it should be tested whether this step is necessary and whether it does increase uncertainty to the product or not.

The most important issue is that the validation of the RF and the pixel based snow depths is not fair. This is because stations used for validation are only a temporal sub-sample of the training station set. The spatial sub-sampling was not conducted, i.e., stations for all geographic locations were used for training and validation. This is a very important problem, because latitude and longitude are the third and fourth important predictors in the model, nearly as important as the Tb. The RF model cannot know values of this predictors already during training, because the validation does not make sense. Therefore, the errors reported in this study are very optimistic (underestimated) and should be recalculated using 50

The pixel based SD product effectively fails to model snow above 20cm depth (Figure 9). This is a serious limitation and it should be explained very deeply in the discussion: (1) why this happens, (2) what is the true applicability of the product given the RMSE is 5cm, what is 25

The methods are very difficult to follow, I noticed that the other Reviewers managed to understand them better than me, but still, I am not completely sure how the study was conducted. This entire chapter should be rewritten, simplified and better structured. Often different words are used in the same context to name the same things, what makes understanding of this paper even more difficult (see attachment for some examples).

The results and discussion sections are very poorly written: methods, results and discussion are mixed in each of these sections (see attachment for some examples).

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Authors should also justify better why this is a real-time approach. Is there an operational implementation of this algorithm?

Eventually, Authors claim that ML in RS is a very novel research problem, e.g. “Machine learning (ML) is a common method used in many research fields, and its **early** application in remote sensing is promising”. The applications of ML in RS are not early, they are in RS since decades, either for regression (as in this study) or classification. The use of RF for regression, cannot be understand as a novelty, because it simply is not. Authors should better explain in which aspects the MS is novel.

Minor issues:

The manuscript is very poorly written with many inconsistencies. I had so many comments that I was not able to put them back from paper to the online reviewing system. Therefore I attach a scan of the paper with hand-written comments. I attach only those pages which had some comments on them.

Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2019-161/tc-2019-161-RC3-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-161>, 2019.

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