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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 basic theme of this manuscript, the application of random forest (RF) to provide an empirical transfer model from remotely sensed radiances to snow depth, has merit, given that physically based transfer models are subject to limitations. However, some of the modeling choices appear questionable and should be better justified or simplified.

The RF modeling described in Section 2.3 has the following main components: (1) Using SSMI data from 1987-2004 for training and from 2005-2006 for validation, in order to evaluate the number of training samples required for good accuracy. (2) Using AMSR2 data from 2014-2015 for training and from 2012-2013 for validation. Snow

depth estimated by this model is then used to generate an approximate spatially varying relationship between 2 SSMI channel radiances and snow depth. The resulting simple SSMI-based formula is used to reconstruct estimated snow depth for 1987-2018, which is validated for 2017-2018.

Approach (2) appears unnecessarily complicated. If the goal is to establish a product for 1987-2018, where only SSMI inputs are available for the entire period, it is more logical to train an RF model directly with SSMI inputs (as done in (1) – not with AMSR2 inputs) fitted to station data (not reconstructed data). If the authors want to retain their more complicated approach, they should compare it to the simpler one to demonstrate that it actually has superior accuracy.

There is another way to tackle the problem of different microwave satellite sensors being available over different portions of the 1987-2018 period, which the authors may also want to consider. This would involve combining estimates from multiple fitted RF models, one for each satellite sensor available for part of the time period, which would potentially more fully use the partly-independent information from multiple satellite sources, which may each have different wavelength ranges, overpass times, and other sensor characteristics.

Another issue is the training/validation station data split. As one of the other reviewers points out, in order to better estimate the error at ungauged sites, it makes more sense to not use some stations at all for training and retain them for validation, instead of validating with data for the same stations but different years.

There is no comparison presented between the RF method and physically based transfer models or existing satellite or reanalysis snow products over China. This work would be stronger if the authors can conduct such a comparison and show whether RF in fact leads to improvements in snow estimation beyond existing approaches.

Section 4.5 discusses the performance of an RF model under an ensemble of simulated weather conditions and microwave radiances. It is not clear what this section adds to the stronger results of the earlier section, which are based on real satellite and snow data. The authors should consider omitting it, and returning to these considerations in a future publication.

Also, the authors should discuss the difference between snow depth and snow water equivalent (SWE). To my understanding, SWE is more relevant for hydrologic applications, and may be more directly measured by the microwave retrievals.

On a related note, the authors note that snow measurements in high mountain areas are sparse, so that remote sensing based snow estimates cannot be validated. This could be partly overcome using a mass balance approach based on, for example, spring and summer streamflow measurements, which would give SWE (and hence, making assumptions about density, also snow depth) on a watershed scale (which in some cases might even be comparable with the satellite spatial resolution scale). See, e.g., Dahri et al. (2018) "Adjustment of measurement errors to reconcile precipitation distribution in the high-altitude Indus basin" and related work.

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