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Interactive comment on "Spatiotemporal variation of snow depth in the Northern Hemisphere from 1992 to 2016" *by* Xiongxin Xiao et al.

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This paper presents a Northern Hemisphere daily snow depth (SD) and snow water equivalent (SWE) product (NHSnow) over the 1992-2016 period, by applying a support vector regression snow depth retrieval algorithm, already published by the same team (Xiao et al., 2018, RSE). This algorithm uses passive microwave (PM) remote sensing (RS) data (SSM/I and SSMIS) and auxiliary data such as in-situ meteorological and snow depth data for training, and an empirical snow density model for SWE retrieval. Only dry snow is considered in this retrieval since it is based on PM data. Performances of this NHsnow dataset against in-situ SD data was compared to those of Globsnow2 (GB) and ERA-Interim reanalysis (ERAi). SWE retrievals were not evaluated. Results show that NHsnow SD is of the same order of magnitude than GB and ERAi for bias, mean absolute error (MAE) and RMSE, expected a slight smaller mean bias of 0.59

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Discussion paper

cm, compared to – 1.19 cm (GB) and 5.6 cm (ERAi). Even if the method used appears interesting (presented in another paper already published), I don't see the real added-value of this dataset? The methods remain dependent on in-situ observations (needed for training), these in-situ data are sometimes sparsely distributed particularly in the North, giving point measurements against 25 km resolution... The known limitations from using PM data (wet snow, deep snow, mountainous area...) are not discussed, and seem not improved? Furthermore, the SWE retrieval is based on an empirical density equation that leads to non-validated SWE values! Thus, the motivations for using such dataset remains unclear given the numerous other databases?

Moreover, the literature review presented for SD and SWE retrievals is incomplete. The authors ignore recent results from assimilation of RS data in Land Surface Model, including improved snow model, driven by meteorological data (and/or reanalysis). Such approaches are more interesting given their independent from in-situ snow measurements and provide both SD and SWE data (See Larue et al., 2018, Hydrol. Earth Syst. Sci., 22; Kwon et al. 2016, J. Hy- drometeorol., 17, 2853–2874; Charrois et al., 2016, The Cryosphere, 10:1021–1038; De Lannoy et al., 2012, Water Resour. Res., 48, W01522). Also recent active PM SAR-based analysis can provide SD data at high spatial resolution : coherence analysis (Singh et al., Water 2020, 12, 21) or phase difference from ESA Sentinel constellation, Leinss, S.; Parrella, G.; Hajnsek, I. Snow height determination by polarimetric phase differences in X-band SAR data. IEEE J. Sel. Top. Appl. Earth Observ. Remote Sens. 2014, 7, 3794–3810), also completely independently from in-situ data!

In their paper, the authors analyzed also the trend of SD (mean and max), SWE, Snow Cover Extent (SCE) and Snow Cover Duration (SCD), showing similar known results than those already published. There are no really new insights here, even if the results are well presented with maps showing spatial variability between North Hemisphere regions (excepted trends slighted over too short periods, see bellow). Also, the authors do not discuss the fact that results based on dry snow only are biased in spring when snow is generally wet. Finally, this paper brings any explanation on the observed trends (some period and areas with increase or decrease snow parameters), as the authors recognized at the end of the paper.

Overall, I recognize that to produce a global dataset is a strong work and that the authors succeed to reach the mean accuracy level of existing databases, but this paper is relatively weak in its original scientific contribution (any real improvement; trends more or less known). I thus don't recommend its publication in TC.

This paper describing the NHsnow database should be submitted to the dedicated journal for new released datasets: Earth Syst. Sci. Data.

Specific comments 1. Introduction: incomplete literature review about other approaches. Also, limitations of SWE retrieval based on PM are not well reviewed. One of the main problem is the snow microstructure (grain size, stratigraphy, ice crust layer. . .) that evolves during the winter and that strongly affects the PM emission, more than SWE! (see Sandells et al.,2017, The Cryosphere, 11, 229–246; Roy et al., 2016, The Cryosphere, 10; Durand et al., 2011, IEEE Geosci. Remote Se., 8 ; . . . and Matzler, 1987, Remote Sens. Rev., 2, 259–387).

3.3 Estimation of SWE Very empirical approach (Eq. 3 and Table 3), and without statistical error analysis? PM data are known to be limited over deep snow (see Larue et al., 2017, Remote Sens. Environ., 194).

4. Results Yes, in-situ SWE datasets exist for data over Siberia (Bulygina, O., Groisman, P. Y., Razuvaev, V., and Korshunova, N. (2011). Changes in snow cover characteristics over northern eurasia since 1966. Environmental Research Letters, 6(4):045204) and over Canada (Brown, R. D., Fang, B., and Mudryk, L. (2019). Update of canadian historical snow survey data and analysis of snow water equivalent trends, 1967-2016: Research note. Atmosphere-Ocean, 1-8).

All the maps are too small, hard to read. Seasonal trend analysis biased when based on dry snow. Have you eliminated wet snow from ERAi outputs?

4.2 Snow mass trend I don't agree with the snow mass trend over too short periods (1992-2001) and 2002-2016) (Fig. 10). A trend over only 10 years makes no sense: you only change one value in the series, and the slope changes drastically! Such analysis has no interest here (maybe for sensationalism public journals!) Analysis of SWE is insufficient.

4.3 Snow cover days: the usually term used is "Snow Cover Duration" (SCD)

5. Conclusion No convincing arguments for using NHsnow instead of others? (added value?, improvements?).

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