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Interactive comment

Interactive comment on "Understanding terrestrial water storage variations in northern latitudes across scales" *by* Tina Trautmann et al.

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The main objective of this study is to analyse the spatial and temporal variability of snow pack and liquid water (mainly soil moisture) components at mid to high Northern latitudes and their respective contributions to total water storage (TWS) variations. To do so, a parsimonious hydrological model adapted to this purpose was first developed and calibrated using Earth Observations datasets, including TWS, snow water equivalent (SWE), evapotranspiration and gridded runoff. A comprehensive description of the model is provided in the Supplementary Material and a rather deep analysis of the calibration procedure is proposed. The authors also made a great effort in analysing the performances of the model at different time scales (seasonal and interanual) and spatial scales (grid pixel and whole domain). Then the model is used over the 2000-2014 time period to evaluate the contribution of solid and liquid water components to

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Discussion paper

TWS variations at these different spatio-temporal scales. Main conclusions are that TWS variations are mainly driven by snow dynamics at seasonal scales, while liquid water dominates TWS variations at interanual scales. Before concluding, the authors discuss some limitations of the method.

The paper is well written and organized. The conclusions are consistent with results presented all along the manuscript. The analysis of the calibration results (in terms of parameter values) is appreciable. Also appreciable is the comparison of the model to state-of-the-art global hydrological models from the eartH2Observe project, showing that despite its simplified structure, the current model performs reasonably well. I have only one major comment and some minor remarks and suggestions developed in the following.

Major comment:

The need to develop a new model is not clearly stated, which is of prior importance since a large part of the paper is devoted to the presentation/validation of this model and the model outputs are used to draw the conclusions. Namely: - why not using existing models that show comparable performances and include more processes? - why not directly compare TWS and SWE from observations used here to calibrate the model? In that case, do the conclusions remain unchanged?

Minor comments:

P1L20: "... observed hydrological spatio-temporal patterns..."

P2L32: Some models explicitly simulate the upper soil layer using a multi-layer scheme (e.g., the ISBA land surface model, Decharme et al., 2011). In that case, satellite-derived soil moisture can be compared to model outputs, and even assimilated with positive impacts on the model performances (Albergel et al., 2017).

Decharme, B., Boone, A., Delire, C., & Noilhan, J. (2011). Local evaluation of the Interaction between Soil Biosphere Atmosphere soil multilayer diffusion scheme using four pedotransfer functions. Journal of Geophysical Research Atmospheres, 116(20), 1–29.

Albergel, C., Munier, S., Leroux, D. J., Dewaele, H., Fairbairn, D., Barbu, A. L., ... Calvet, J.-C. (2017). Sequential assimilation of satellite-derived vegetation and soil moisture products using SURFEX_v8.0: LDAS-Monde assessment over the Euro-Mediterranean area. Geoscientific Model Development, 10(10), 3889–3912.

P4L5: Which datasets are used to mask out such pixels?

2.2 Model description: if I understand correctly, incoming water from upstream grid cells are not accounted for. At the monthly time scale, I agree that this would be negligible at the pixel scale, but is it still true at the basin scale (e.g., the Ob river basin)?

P6L8: "... daily cumulated gridded precipitation..."

P6L11: "... that combines remotely-sensed precipitation..."

P7L2-4: Are these data assimilated into a snow model?

2.3 Input Data: Since EO uncertainties are an important aspect of the calibration process, I suggest the authors to add a figure showing maps of temporal averages of each uncertainties for each dataset. This could help interpreting the model performances as shown in Figures 3, S1 and S2.

P10L8: "Therefore..."

P11L4-9: This paragraph is unclear. Is it related to the smoothness of GRACE spatial patterns? In that sense, I think that for a better comparison with GRACE, modelled TWS should be first processed to remove high frequency spatial variability that is not observed by GRACE.

P11L22-24: Is the overestimation found by Behrangi et al. (2016) and Swenson (2010) quantitatively comparable to this study?

P11L26: "... if SWE > 80 mm (parameter snc)."

3.1 Model optimization: It would be interesting to discuss the values of the four cost terms in Eq. (2) obtained with the optimized parameters.

P14L12: Are "seasonal variations" equal to the "mean seasonal cycle"? We understand after (from the figures) that yes. In this case, very high correlation values are not really surprising. Bias and RMSE would be more suited.

Figure 3: It seems from figure 3(d) that large RMSEs are found in regions affected by the Postglacial Rebound (Eastern Canada and Scandinavia) and near coastlines (ocean signal contamination?).

P19L5: Do the authors have any possible explanation of the large negative anomaly in 2003 and why it is not captured by the model?

P23L4: The average value does not show that CR is positive over the entire domain.

P23L14: "... less variable at interanual time scale..."

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