Geoscientific Model Development Discussions

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Comment on gmd-2020-434

Anonymous Referee #2

Referee comment on "Climate-model-informed deep learning of global soil moisture distribution" by Klaus Klingmüller and Jos Lelieveld, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-434-RC2, 2021

This study explores DNN-based soil moisture predictions that are trained on remote sensing data and forced by a climate model, which are used to replace low-quality soil moisture predictions in that model for an improved prediction of mineral dust emissions.

Overall, the topic is relevant, the methodology novel and sound, and the manuscript concise, well structured, and was a pleasure to read. I just have a few concerns and suggestions which I hope the authors will consider before publication.

-) To me, hardware developments do not seem to be the most pressing issue and motivation for the presented study. I would recommend to start the introduction outright with the more relevant issue, i.e., the low-quality internal soil moisture predictions of the climate model which could potentially be replaced by a DNN-based module.

-) Maybe a better way to achieve independence between training and test data than doing a standard 80/20 splitting could be to take the characteristics of the ESA CCI SM product into account, that is, splitting the data at a time where there is a change in the underlying satellite instruments that are being merged. Or perhaps better yet: training the model on the "active-only" ESA CCI SM data set, and evaluating it on the "passive-only" data set (but still in a different period of course). That's probably too much to ask for this paper, but perhaps something to keep in mind for future studies.

-) What motivates the selection of the predictor variables? Are they taken from another study, or did you try out different combinations and evaluated variable importance? A little elaboration on that would be helpful.

-) On a related note: I am a bit concerned that the DNN predicts mostly a spatial and

temporal climatology. The correlations of >0.9 are arguably unusually high for typical soil moisture data sets. This argument could be settled, for example, by showing anomaly correlations as well. As for the temporal correlations: The values of ~0.5 indeed appear to be more realistic, but I don't understand the reason for not showing the acutal values in Figure 4. A discussion on how these temporal correlations compare to values typically found for other data sets could also be beneficial. For example, Figure 7 in Dorigo et al. (2017) shows correlation coefficients of modeled versus ESA CCI SM soil moisture, which may serve as a good benchmark for putting the attained values into perspective. Another way to appease the reader may come from an elaboration on the selection of predicture variables, as noted above... For example, how would the predictions look if merely coordinates and the time of the year would be provided? Or if they would be omitted? Also, the time series shown in Fig. 5 do not really convince me. For example, it is pointed to "irregular features" in, for example, October 2014. But to me it seems that there are distinct dry-downs in the prediction in both October 2014 and October 2015, while this dry-down is only visible in the observation in 2014.

-) Having said that, for the application shown, it seems that this may not be relevant at all. The improvements in dust emission predictions shown in Figure 6 are quite remarkable, and it seems that the DNN-predicted values, even it they would be only a climatology with possibly a little short-term variability signal added on top, are doing already much better than the model-internal soil moisture representation. The taken approach of validating soil moisture predictions using a downstream application is a great way to show their actual utility, which is much more useful than the more common approach of simply looking at rather meaninglesss correlation values alone.

-) I'd change the title of Sec. 4 to singular since only a single application is shown.

-) I recommend to add a note of caution to the argument that the DNN could be used to fill gaps in the remotely sensed data. For example, tropical rain forest are masked out entirely in the ESA CCI SM. I am not sure if the DNN is able to properly learn the relation between the predictor variables and soil moisture in such a distinct regime if it is not represented in the training data set at all. A similar argument can probably made for the winter. If soil moisture cannot be retrieved because the soil is frozen or covered with snow, then I would not expect a DNN to properly turn precipitation, which is most likely in the form of snowfall, into accurate predictions of soil moisture.