Geoscientific Model Development Discussions

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Comment on gmd-2020-434

Anonymous Referee #3

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-RC3, 2021

General Comments:

This paper presents a deep neural network model that predicts global surface soil moisture from precipitation, temperature, and humidity outputs from a climate model. The model was trained on daily satellite retrievals of soil moisture. The authors suggest two uses for the model: 1) to provide modeled soil moisture inputs for related applications, and 2) to fill missing values in satellite retrievals. The authors demonstrate an application by simulating threshold surface friction velocity for mineral dust emission in the Arabian Peninsula and Mesopotamia.

I found this paper to be rigorous, complete, and convincing. While I have some questions, I think the overall quality is very good. The conclusions are well-founded, and the future research questions are well discussed.

This paper could use some help from an English language editor. Some sections of the paper are very well written, and some have grammatical and language flaws. Even so, the paper is easy to read and understand.

Specific Comments:

The authors present a simulation of threshold surface friction velocity for mineral dust emissions in the Arabian Peninsula and Mesopotamia as an application of the model. They simulate the threshold friction velocity using both observed and DNN-modeled soil moisture with good agreement. This leads me to ask: why use the DNN here at all? Why not just use the observations directly?

The authors say that Figure 6 shows "The results based on the observed and predicted soil moisture show good agreement and a strong seasonal cycle". More detail should be given here. The model appears to overpredict the threshold surface friction velocity a bit in the summer. Then, "whereas the result based on the EMAC soil water has little variability" – they could also compare the DNN to EMAC soil water directly.

The authors should provide more information about the model selection criteria they used for the DNN input variables. They used a set of 18 input variables, all of which are intuitive. However, it would be interesting to see which of these input variables drive the predictive power of the model. This might be a particularly interesting question for the mineral dust application: what are the most important drivers of soil moisture in the Arabian Peninsula and Mesopotamia and what does this mean for vulnerability to dust storms? In regions where the temporal correlation is weaker, are the cos(2*pi*t/a) and sin(2*pi *t/a) terms dominating to impose the observed seasonal cycle?

Figure 5 shows a time series comparison of predicted and observed soil moisture at a single pixel during the test period. This pixel is located in Germany, where the model is reported to have strong temporal correlation (Fig. 4). What does the time series look like in a pixel with a poorer temporal correlation? What does is look like in a pixel in the poorly correlated region of the Arabian Peninsula?

Figure 7 shows the global distribution of observed and predicted volumetric soil moisture on two days in the training period. It would be very interesting to see similar plots for the test period.

Technical Corrections:

There is quite a bit of model evaluation work in the "Applications" section. In particular, I think that the presentation and some of the discussion of Figures 5 and 7 could be moved up.