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Discussions

Interactive comment on "Global Downscaling of Remotely-Sensed Soil Moisture using Neural Networks" *by* Seyed Hamed Alemohammad et al.

Anonymous Referee #1

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The manuscript presents a new global soil moisture product provided at an unprecedented spatial resolution of 2.25 km. It is built from a neural network (NN) and data comprised of SMAP 36 km resolution level 3 soil moisture, an enhanced soil moisture product derived from 36 km SMAP observations and posted on a 9 km resolution grid (Chan et al. 2017) and MODIS NDVI data aggregated at various resolutions between 2.25 and 45 km. The authors have also tested the inclusion of a topographic index at the target downscaling resolution. The approach is evaluated by analyzing global soil moisture maps and by comparing downscaled soil moisture estimates against in situ data from the international soil moisture network (ISMN).

A global soil moisture product at 2.25 km resolution is of high interest to the hydrological and Earth system science community. I also find that the comparison of the

NN method with simpler methods (linear interpolation, and the null-hypothesis i.e. no disaggregation) is quite positive as well. In fact, my comments mainly concern the underlying assumptions of the approach (comments #1 and 2) and the evaluation of the downscaled data set (#3).

1) On the use of the 9 km resolution soil moisture product. The basis for the proposed approach is to calibrate a relationship between 36 km (SPL3SMP) and 9 km (SPL3SMP_E) resolution soil moisture products, and then to apply it at 9 km resolution to derive the 2.25 km soil moisture. The point is that the actual spatial resolution of SPM3SMP_E (the so-called "9 km resolution product") is 33 km while it is resampled at 9 km resolution (Chan et al. 2017). The 33 km resolution is so close to the original 36 km resolution SMAP level 3 data that one may wonder how a relationship derived from 36 and 33 km resolution data can be valid between 9 km and 2.25 km resolutions. At the very least, I recommend a sensitive analysis to assess the impact on the results of a coarser spatial resolution (33 km instead of 9 km) for training.

2) The NN is trained and run using NDVI data as auxiliary information about the subpixel soil moisture variability. Some limitations related to the soil moisture-NDVI relationship are mentioned in the conclusion (such as presence of vegetation, saturation effects). However, I think that the discussion should be deepened. It is true that NDVI and topography are variables available at global scale, but they are not the only factors explaining the soil moisture variability. In addition, the soil moisture-NDVI relationship established at the monthly time scale (phenological time scale) may not be valid at the daily time scale, at which SMAP observes the Earth and the observed surface soil moisture evolves. For clarity, the assumptions underlying the implementation of the NN using NDVI data should be better highlighted in the manuscript.

3) Evaluation of the NN output: Line 22 page 9: "NN is appropriately explaining the spatial variability of soil moisture using NDVI as ancillary data". Line 14 page 13: "our evaluation shows that the downscaling algorithm has high accuracy in terms of temporal correlation, anomaly correlation and ubRMSE when compared to in situ soil

moisture estimates from ISMN". It is difficult to assess the quality of the downscaled soil moisture at fine scale using global maps. Global maps convey the message that the high-resolution product is global, but some fine scale assessment is missing. Evaluation of the results over focus (perhaps instrumented) areas would be very useful. Regarding the temporal aspect, validation using 2-year averages does not allow for assessing the relevance of soil moisture-NDVI relationships at the temporal scale of SMAP observations/surface soil moisture dynamics. In addition, I do not think that the comparison with in situ measurements shows "that the downscaling algorithm has high accuracy". I would soften this point of view as results are very similar for all products (from 2.25 km to 36 km, see Figure 8). Even though the downscaling method does not degrade low resolution information, the improvement is hard to detect. The authors mention that "accuracy is better than or equal to the SMAP 9 km soil moisture estimates". I take them at their word, but from Figure 8 it seems that the original 36 km product be more accurate at several stations. More explanations are needed to clarify the improvement provided at 2.25 km and at which temporal scale.

Specific points:

a) I may have missed something but the two statements re-written below look contradictory: - Page 9, line 11: "Moreover, the latitudinal average plots (on the right side of each panel of Figure 5) show that at higher spatial resolutions there is more spatial heterogeneity. The latitudinal average for the 36 km product is much smoother than the 2.25 km one." - page 9, line 18 : "For comparison, we also calculate CV for the 9km soil moisture estimates from SMAP at the 36km grid (Figure 6 bottom panel). The two panels in Figure 6 have different range of CV which is expected given the difference in their spatial scales." Since aggregation tends to reduce variabilities, one would expect an increase in the spatial variability at higher spatial resolution. However, the CV is divided by about 5 at 9 km resolution compared to the CV at 36 km resolution. Could the authors comment on their seemingly opposite findings ?

b) Line 13 page 2: "Some of them use linear relationships (i.e. projection) to define

the impact of spatial heterogeneity using ancillary data, typically in combination with a radiative-transfer model to relate surface temperature and soil moisture (Colliander et al., 2017a; Merlin et al. 2005, 2008a, 2008b, 2008c)." I noted two errors in this sentence: 1) physical models that relate surface temperature and soil moisture are energy balance models (not radiative transfer models) and 2) the projection technique used in Merlin et al. 2005, 2008a does not implement linear relationships, but a non-linear energy balance model.

c) Line 15 page 2: "A major issue is that surface temperature at finer spatial scales from satellites cannot be estimated under cloudy conditions". Agree and I would add another essential limitation that the surface temperature cannot be used as a signature of soil moisture in energy-limited conditions.

d) Line 21 page 4: "we assume that the scaling relationship between 36 and 9 km soil moisture estimates is the same as the scaling relationship between 9 km and 2.25 km resolution. To the best of our knowledge, this is the first time that the assumption of similar scaling relationship is used to downscale soil moisture". I would like to mention that the same scaling relationship has already been used to downscale soil moisture from 40 km to 1 km and from 1 km and 100 m in Merlin et al. 2009 and Merlin et al. 2013. Merlin, O., Al Bitar, A., Walker, J. P., & Kerr, Y. (2009). A sequential model for disaggregating near-surface soil moisture observations using multi-resolution thermal sensors. Remote Sensing of Environment, 113(10), 2275-2284. Merlin, O., Escorihuela, M. J., Mayoral, M. A., Hagolle, O., Al Bitar, A., & Kerr, Y. (2013). Self-calibrated evaporation-based disaggregation of SMOS soil moisture: An evaluation study at 3 km and 100 m resolution in Catalunya, Spain. Remote sensing of environment, 130, 25-38.

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