Review of the manuscript egusphere-2024-170, Depth-extrapolation of field-scale soil moisture time series derived with cosmic-ray neutron sensing using the SMAR model by Rasche et al.

Summary

The Authors explore the possibility to estimate deep soil moisture (SM) from cosmic-ray neutron sensing (CRNS) observations based on the SMAR model. A good data-set from a point-scale soil moisture network (SMN) is used as a benchmark. Several alternatives are explored by combing different CRNS-based SM and the SMAR models settings. The results show that the CRNS- SMAR approach is not able to reproduce depth-extrapolated SM well, but it is argued that it is in line with previous studies (i.e. RMSE < 0.06 m3 m-3).

The manuscript is well structured but not always clear. Some additional information should be reported. Several analyses have been performed but I did not recognize a clear design to address the research question. Many factors are in fact combined and it is not clear if the performances depend on the CRNS, on the unrepresentativeness of the SMN, or on the SMAR model. So, in my opinion readers are stuck on the outcome of the study and how to proceed. Overall, I believe that the Authors should put major effort into improving the manuscript for possible publication. Below I provide more details to clarify my arguments.

Major concerns

[1] About the use of point scale soil moisture network (SMN)

The soil moisture network (SMN) represents a valuable data-sets (L131-132). Still, it is explained that could not represent CRNS footprint well (L348). Considering in addition that the deeper soil moisture network (> 40 cm) is based on a lower number of point-scale sensors, it could be questionable its value as benchmark for testing the performance of the depth-extrapolated CRNS-SM. For this reason, it cannot be concluded in my opinion if the difficulties to estimated deeper SM are based on CRNS settings, SMAR settings or an unrepresentativeness of the SMN. To circumvent this issue, I strongly recommend the Authors first to test SMAR settings only to the SMN, i.e., using first layer of SM measured by the SMN to estimate the deeper SM. Then, to apply the best settings of the SMAR model to CRNS. This exercise could be performed to each single soil moisture profile and any combinations. The results should provide a benchmark for testing the depth-extrapolated SM based on CRNS. Please note that you have already quantified some differences between CRNS and SMN. Thus, it should be expected that the depth extrapolated CRNS-SM cannot perform better than that. Finally, it is not clear to me if the depth extrapolated CRNS is compared with the weighted SMN or to the arithmetic average SMN. It should be the former to be consistent with the signal but with the disadvantage to have again weighted soil

moisture. This issue seems to not be resolvable and could limit some applications. Discussion should integrate this aspect.

[2] About the use of SMAR

First, I found misleading the arguments to support the use of SMAR. The Authors argue several times that "most approaches require a site-specific calibration using depth-profiles of in-situ soil moisture data, which are often not available." (L6-7). For this reason, you use SMAR. However, this approach also requires calibration. So your arguments do not support your decision. Similarly, it is stated (L9) that "SMAR is usually also calibrated to sensor data, but could be applied without calibration if all its parameters were known" and L90. "This method does not require calibration if the environmental parameters are known." Well this is valid for any models and not only for SMAR. The problem is that it is often the case that we do not know the parameters, and especially at the scale of model application when soil is under investigation. The results of this study also confirm this statement. So your arguments do not support your decision to use SMAR as many other approaches have similar settings. Overall, I'm fine with the decision to test SMAR (L86). But I'm against the argument that this is physically based and it does not require adjustment in comparison to other approaches. The results of the present study also confirm my doubt. Comparison to other approaches, e.g., the exponential filter (Franz et al., 2020; Wagner et al., 1999) could strengthen this study.

Finally, the Authors tested several SMAR settings. Despite the overall exercises are remarkable, I'm wondering why testing approaches that already sound not appropriate. I.e.,I found the assumptions behind the original SMAR model very arguable. E.g., a constant flux V2 (P and ET) is already debated when applied for estimating monthly groundwater recharge and it should be considered unrealistic over a long time series (years) for hourly to daily resolutions. The results show how V2 moves from, e.g., 50 to 300 mm h-1 in Table 3. These results are not consistent and support my doubt of using standard approach. So I'm not surprised if the Authors tried to modify the approach and I would remove the scenario with the constant V2. Similarly, the CRNS effective depth seems to be easily estimated (L279). So why testing a guess of 35 cm? I would remove this scenario. Overfitting a model calibrating all the parameters also seems to me a crash test and it does not seem to me a good approach for testing if a model is reliable. Several parameters are not in a physically consistent range and what was argued to be a physical approach has been destroyed. I would also remove this scenario.

Specific comments

L3-5. deeper than what? I would rephrase otherwise it is not clear to what you refer when saying "deeper", e.g., moving the sentence "Many of these applications require information on soil water dynamics in deeper soil layers." After " Cosmic-ray neutron sensing (CRNS) allows for non-invasive monitoring of field-scale soil moisture across several hectares around the instrument but only for the first few tens of centimeters of the soil."

L8. Personally, I'm not too much on the philosophical discussion about process-based or empirical approaches (e.g., (Hrachowitz and Clark, 2017)). But to call a bucket model with several empirical assumptions a physical-based approach could be highly criticized. Please consider rewording.

L36. Despite, I agree that establishing an extensive point sensor network requires a lot of effort, the international soil moisture network ISMN (Dorigo et al., 2021) provides a good example that it is worth citing.

L80 and L82. To my knowledge, (Wagner et al., 1999) proposed the use of an exponential filter approach to estimate soil water content at deeper soil layers based on surface soil moisture. Instead the term SWI refers to a quantity of water content between 0-100%. Thus, I would not call the extrapolation approach "soil water index" but rather "exponential filter approach".

L128. How field capacity and wilting point have been estimated is missing and should be added.

L134. The use of the manufacture's calibration function could be another source of errors that should be discussed in the result section.

L154. The fact that you use a 25h moving average could be a reason to not be able to capture fast soil moisture changes? This could be discussed in the result section.

Eq.5. I guess h is air humidity. Please clarify.

L179. Please clarify how look-up-table approach works. What are also the assumptions/approximations in using this approach?

L181. So how Nd has been actually calculated? Is it related to N0? Please clarify.

L187. What are the differences between URANOS type and MCNP type equation?

L211. Are you sure you have to move back and forward from volumetric soil moisture and relative saturation? Could you not try directly expanding eq. 7 directly for volumetric soil moisture?

L248. I guess the estimation would be very sensitive to the length of the time series. Did you use the entire time series for estimating ET? Please clarify.

L490. Well, the discovery that root water uptake varies with time and depth depending on the water availability goes probably back to the introduction of irrigation practices in the history. I guess Maysonnave et al.,2022 showed more than that. If the Authors remove the scenario with V2 constant, this comment could also be probably removed. Otherwise please rephrase.

The Authors have stated that this study is the first who evaluates the UTS (L515). If this is the case I think is worth clarifying this from the beginning and provide some additional information to better understand how it works and is implemented.

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