

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2020-611

Anonymous Referee #2

Referee comment on "Comprehensive evaluation of satellite-based and reanalysis soil moisture products using in situ observations over China" by Xiaolu Ling et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-611-RC2>, 2021

This interesting analysis used in-situ observations in China to evaluate several reanalysis- and RS-based SM products. While it is a nice self-contained study with seemingly comprehensive analyses, I found the study lacking sufficient physical explanations supporting several findings of their analyses. Also, some figures are not very well presented and need to be updated. Therefore, I'd suggest the authors go through moderate revisions before this paper can be publishable. Below are some suggestions to improve the paper:

Insufficient explanations/supports:

- 6: ESA-CCI seems to not represent seasonality well. Why? It seems no variation there. I think this explanation on "which may be because of snow or frozen soil during these periods" is too thin. To me this still does not explain well on why worst seasonality are there.
- 8: it seems discouraging that none of the products available captures the anomalies well especially in NC. Can the author provide some feasible explanations on why this is the case, and discuss how this could influence applications in those regions and what are the potential future directions for improvements?

- Line 301: I think “which is partly due to the combined influence of longwave and shortwave radiation” does not sufficiently explain why low correlation there. Please expand what you mean exactly. Also, if separation of LW and SW radiation helps, would it be possible to use LW and SW data to re-draw this scatter plot?

- 12 & L298-L302: overall I think it’s an interesting figure. However, authors fail to explain in more detail on the underlying physical mechanisms responsible for these correlations and why they wanted to perform these analyses. This paragraph is too thin. In addition, It seems these plots are more driven by the availability of data, instead of driven by hypothesis testing needs. It would be helpful for the authors to put more thoughts on this figure and provide readers with more insights on why they chose to do the analysis and what’s new after doing the analysis.

Figure presentation problems:

- It is very difficult to distinguish in-situ line in Fig. 6 as it can be confused with ERA-5. I’d suggest to use thicker black line to denote in-situ observations in Fig. 6. Also, be better to use consistent legend with Fig. 4 & Fig. 8.

- 7: I think it would be very difficult for readers to directly extract useful information from this figure, partly because of the color bar used, which makes it all red (plus there are so many panels). I’d suggest to use more continuous colors, with more contrasting

from 0-1, such that differences in the correlations are better presented. Since only very few locations show negative correlations, you can cap the lower bound at 0, and just mention "limited negative correlation" in the caption. This way, 0-1 can be better contrasted (using blue to red) to support your interpretation on the figure in the main text.

- 10 & Fig. 11: the caption is incomplete and misleading. It did not mention which skill metrics is plotted here. Please mention it explicitly in the caption. Also, draw a reference line on 0 such that readers know where to expect good performance.

Minor:

- I do not think the literature review is comprehensive. Beck et al. (2021; HESS) presented a much more comprehensive study performed at the global scale. It should be included in the Introduction and discussions on relevance to your study needs to be mentioned. I disagree with the claim in L62 that no long-term SM products have been compared with ESA-CCI. Please revise accordingly.

Overall comment:

- In fact, I like the study very well because it is self-contained, with comprehensive analysis, and the writing is good too. However, I am thinking what could be more useful to the community, is perhaps for the authors to share their in-situ soil moisture observations through posting the data via figshare or other publicly accessible data portal. It seems to me that this study is only unique because of its observations, which are generally not shared with the public. If the data can be shared properly with the whole community, people may find more innovative ways of using the data for other research purposes such as drought monitoring. Is this something that the authors are considering? It could be helpful to at least comment on or discuss this issue in an academic paper.