

Interactive comment on “Combining satellite observations to develop a daily global soil moisture product for a wide range of applications” by M. Enenkel et al.

M. Enenkel et al.

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Dear reviewer #2,

Thanks a lot for your valuable comments. We appreciate your effort and the intention to improve both the quality of the manuscript and its suitability for HESS. Please find our reply to the comments below.

1) The “main findings” listed in Section 5 read more like internal technical notes for the CCI RT development team than findings appropriate for a peer-reviewed publication (especially the first one and the last one). Why are these findings of interest to a

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broader audience?

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2) Provide a clearer description of how the RT soil moisture retrieval algorithms actually differ from their retrospective equivalents in the existing “research” ESA CCI product. As currently written, the manuscript describes these differences only in very high-level terms. Therefore, it’s difficult for the reader to get anything out of the conclusion that “the research and near-RT products do not differ much” when we really don’t understand the underlying retrieval algorithm differences. For example – if the algorithm differences are relatively small – then this conclusion seems almost trivial. I understand that these differences might be highly technical, but some context is needed for the reader to extract anything meaningful out of the manuscript’s comparisons between the ESA CCI and CCI RT products.

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3) Provide more finality with regards to the details of the near RT product. The paper could also be of external interest as a technical document describing a completed (and ready for use) near RT product. However, the authors seem to be hedging on how close the product is to actual operational implementation (e.g. they state in the last line of the abstract that the product is “getting ready for operational use”). Of course, this timing may be something beyond the author’s control. But, at the very least, they could provide a fuller discussion of the products near RT attributes (e.g. where it will be posted, in what format and – most critically – at what temporal data latency). That way, it can make a firmer technical contribution by helping users better prepare for its eventual availability. This detail is missing from the current manuscript. In short, I recommend that the author’s revise their manuscript to better articulate a clear scientific and/or technical contribution to an external technical audience.

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Additional points: 1) The key issue here is data latency, not temporal frequency, so that title should be changed to reflect this. Replace “daily global” with “global near real-time” in title?

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2) I'd rethink the last sentence of the abstract...it should reflect the key results presented above...maybe something like “In summary, the CCI NRT product is expected to be nearly as accurate as the existing ESA CCI SM product and, therefore, of significant value for operational uses such as...”

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3) Line 25 “per mille” is not commonly used in English...it also not clear what the fraction actually represents the total contribution of soil moisture to all water or fresh water storage or non-ice fresh water storage volumes? Consider re-phrasing and clarifying.

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4) Section 5 – first sentence. The issue is not the performance of “operational” sensors, the issue is the performance of “operational” retrieval algorithms. Considering re-wording this sentence.

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5) Section 5 (p 11562) – lines 24-26. Basically, that author's are suggesting a role for non-stationarity in the GLDAS/AMSR2 rescaling statistics (such that the GLDAS rescaling parameters sample <2013 and applied in the ESA CCI SM are not applicable in the current product). Two points here: first, it's not clear how re-scaling statistics can impact correlations results (res-scaling is a linear operation which should impact correlation attributes). Second, it would be relatively straight forward to look for evidence of this non-stationary. Non-stationarity in rescaling statistics is a major challenges in near RT soil moisture production. Expanding a bit more on this would help the technical contribution of the paper (see my major points above).

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6) Section 5 (p 11563) – lines 10-25. This discussion refers to differences (in e.g. AMSR-2 product versions) that are of very narrow technical interest and would seem more appropriate for a internal technical discussion (rather than an external journal article). Can these conclusions be broadened to be of more general interest?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11549, 2015.

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