

Interactive comment on “Evaluation of 18 satellite- and model-based soil moisture products using in situ measurements from 826 sensors” by Hylke E. Beck et al.

Anonymous Referee #3

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This is a very interesting and promising paper certainly useful to document the bibliographical effort on soil moisture evaluation. I feel however that authors have skimmed over some essential explanations and was sometimes wondering if I had the latest version of the manuscript from HESSD (?). The bullet points format of the manuscript does not help and a lot of discussion is missing prior it can be considered for publication. I recommend major revisions, please see below an attempt to help.

Although very important, this kind of evaluation is by design almost never in favour of the satellite based products. It has been highlighted several time in the literature in the past decade that in data rich areas where models are highly constrained by high

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quality observations, their soil moisture is of better quality than the one retrieved from spatial remote sensing. As the in situ measurements sensors you are using are largely located in those data rich areas, this should be emphasized in the manuscript.

Page 1, Lines 9-8 : a) It gives the false impressions that data assimilation brings an improvement going from 0.69 to 0.72 while models with data assimilation do not all have open-loop counterparts (the opposite being true as well). I know it is the abstract but perhaps you should already give scores that can highlight the added value of data assimilation by considering the mean R values of their open-loop counterpart (HBV+ERA5, HBV+IMERG, HBV+MSWEP). b) I am personally not a big fan of such a statement in an abstract and I am not sure it is well supported by your results particularly regarding the large distribution of your scores (boxplots of figures 2 & 3) and the lack of discussions on score difference significance

Page 1, Line 14 (also Line 16 and true for many parts of the manuscript): Are those differences significant? why didn't you provide confidence intervals? Also according to figure 2 it is ESA-CCI_SWI that has a median R value of 0.67 while ESA-CCI has a median R value of 0.56, please clarify. The notion of with/without SWI does not appear in the abstract (?).

Page 2, Line 14 "Additionally, many had a regional (sub-continental) focus [...]" I would not say yours is different (?) Particularly looking at figure 1, please clarify. Also you could add a lot of recent references that had looked at very similar datasets to like yours. You are only slightly discussing towards the end of your manuscript, please revise

Page 2, Lines 25-26 "Furthermore, several new or recently reprocessed products have not been thoroughly evaluated yet, such as ERA5 (Hersbach et al., 2020), ERA5-Land (C3S, 2019), and ESA-CCI V04.4 (Dorigo et al., 2017)." For ERA5, Li et al. have used 842 qualified sites covering 25 networks (rather recent paper I must admit): <https://rmets.onlinelibrary.wiley.com/doi/10.1002/joc.6549>

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For ESA-CCI, Did you check the product website and documentation? <https://www.esa-soilmoisture-cci.org/validation>

Page 3, Line 1 “[...] from 826 sensors located primarily in the USA and Europe [...]” Thus as for previous studies you have mentioned the extent to which your findings can be generalized is unclear (?), please revise as this sentence could be misleading.

Page 3, Line 5 Question on SWI appears only here, seems a bit out of the blue (?) please introduce SWI earlier not to confuse readers.

Page 3, Line 14, section 2.1 I am wondering here if I have the correct version of the manuscript as several dataset are not presented? It is a general comment that you have to justify why you have used those 18 dataset and not others, otherwise it looks like cherry-picking. While some are state-of-the arts, others are self-made, please revised the choice and presentation of the dataset.

Page 3, Line 24 I assume you have used soil temperature of the first layer of soil between 1-7cm, is so please say it. Alternatively you could have discarded in situ measurements of soil moisture when associated measurements of soil temperature (if available) was < 4 dC

Page 4, Lines 16-17 Add references I appropriate

Page 5, Line 10 “The model was run twice for 2010–2019 [...]” Please clarify if this was done for each forcing dataset (I assume so) Page 5, Line 20 “We calibrated the 7 relevant parameters of HBV [...]” This will have to be discuss further already if it impacts your results wrt to the land surface model based product?

Page 6, section 2.5 Are they all using the same measurement methodology?

Page 7, figure 1 In such study this kind of global maps tend to show areas with no data more than areas with data. It is not obvious than 2 two zooms over North America and Europe add anything, perhaps you could have one figure with 3 panels, North America, Europe and Australia (?)

Also I suspect here that most of the stations in the "cold" class over North America are from the SNOTEL network located in mountainous area where the retrieval of soil moisture from space is rather complex. This should be emphasise in the text at it is biasing your results.

Page 9, figure 2 I may have missed a point but I did not understand how did you obtain 3-hourly data for e.g. ASCAT, SMOS, ESA-CCI, SMAP...please revise.

It would have been easier to have them close to one another (SWI and not SWI) on your figures but has you have several questions to answer it was probably not easy to pick up the correct order of products for those figures.

Page 11, section 3.2 My personal opinion is that this is a low pass filter smoothing the time-series, nothing more

Page 11, section 3.3 Are you R values significant? I may have missed something here but from your figures 2 and 3 (boxplots distribution) it is difficult for me to give a clear answer to this question (while you are doing it in the abstract)

Page 12, section 3.4 Line 21 "[. . .] the central Rocky Mountains [. . .]" This are usually area where it is difficult to retrieve soil moisture form space. Memo perhaps does better than ESA-CCI but is it good? are we talking about R values going from 0.2 to 0.3 or from 0.6 to 0.8? From figure 4 it is difficult to see anything (at least to me). Again, are the differences significant? Lines 22-23 Confidence interval would help Line 29 Please clarify "[. . .] from the best sensor each day[. . .]"

Page 12, Lines 31-32 Is it surprising to find the 3 calibrated HBV models leading this ranking? Again I would not claim such a best to worst ranking without discussing the significance of scores.

Page 13, figure 4 (also true for figure 5) Not sure this figure is very helpful as hardly visible (?) Perhaps you could use scatterplots, e.g. x-axis R for ESA-CCI vs in situ, y-axis R for MeMo vs in situ and then use color-codes for any classification you like.

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Page 13, Line 1 ERA5 is a coupled land atmosphere system where ASCAT has been assimilated. Could you comment on the impact it may (or may not) have when using it to force HTESSEL land surface model in ERA5-Land? is it fully independent from ASCAT?

P.14, Line 1 Data intensive models could also be calibrated don't you think? I personally thing it is wrong to oppose land surface model and calibrated hydrological models. their objectives are different.

P.14, Line 8 There is more to say from such figure as figure 5 (?) e.g. discuss the geographical patterns

P.14, Lines 21-23 Please discuss if it is likely to be because of the inputs quality (ASCAT/SMOS) or a methodological matter.

P.14, Lines 26 There is also a study showing that the assimilation of ESA CCI in GLEAM leads to a decrease of quality (Brecht et al., 2018 GMD?)

Page 14, Lines 32-33 Which was expected right?

P.15, section 3.7 Perhaps this could be moved few sections above?

P.16, Lines 16-19 In agreement with many previous studies (e.g. Albergel et al., 2010, HESS, Dorigo et a;., 2017, RSE...)

P.17, section 3.9 Perhaps worth referencing / discussing Reichle et al., 2019 ? Verification of the SMAP Level-4 Soil Moisture Analysis Using Rainfall Observations in Australia, <https://ieeexplore.ieee.org/document/8898398>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-184>, 2020.

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