

First, I think the authors have revised this paper well. I am satisfied by their responses to my queries – for example the demonstration that the choice of mean or median is not material. The abstract is definitely better. I am also satisfied how they have modified the manuscript. My original review was “major revisions” with the caveat that the changes were important, but unlikely to be overly dramatic in terms of changes in the text. The changes reflected in the manuscript are more than I anticipated but welcome.

Perhaps most critically, I think the authors have edited this manuscript well, it flows more elegantly, the sentence structure is definitely better, the discussion is clearer and I feel the conclusions are now appropriately stated.

I was amused by the response to my comments about the use of *mrsos*. The statement:

However, due to the long timescales considered (1978 - 2000) for both the modelled and empirical data, the average surface soil moisture will be closely related to deeper moisture.

Prove it ☺ Or, delete this sentence. I do appreciate the additional text (except this last sentence) – I think it is useful, and just as useful if you omit this final sentence which will be true in many regions, but not in some that are particularly interesting. This is simply a limit to your study, a limit that could not really be avoided. Just be up-front about it.

The statement:

There is also a question of what soil moisture in LSMs represents, and the definition of soil moisture varies between models.

I fully agree - the famous paper about this, which you could cite, is:

- Koster, R.D., Z. Guo, R. Yang, P.A. Dirmeyer, K. Mitchell, M. J. Puma, 2009: On the Nature of Soil Moisture in Land Surface Models. *J. Climate*, **22**, 4322–4335. doi: 10.1175/2009JCLI2832.1

I’d re-state my previous comments that this paper is well-suited to this journal. It is not a model development paper and so I respectfully disagree with Reviewer 2. For the record, I read the authors responses to reviewer 2. There places where the authors effectively say “we cannot do that because of CMIP protocols” around data reported and so on. I did not see anything in these responses that I disagreed with.

In summary, I think this is a thorough revision that accommodates my earlier comments.