

## ***Interactive comment on “Assessment of soil moisture fields from imperfect climate models with uncertain satellite observations” by G. Schumann et al.***

**G. Schumann et al.**

guy.schumann@bristol.ac.uk

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Reply to comments from Anonymous Referee #2

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General comments:

This manuscript aims to address an important problem - how to evaluate coupled climate models with explicit consideration of both model and observational uncertainty. The proposed technique has the advantage of simplicity, but the manuscript is significantly under-developed. I would suggest the authors either focus on a more thorough

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theoretical investigation or justification of this approach or provide a more detailed example of its application. As things stand, the definitions of uncertainty are very simplistic and the untreated uncertainties in the example given mean the conclusions drawn are only very weakly supported. An expanded manuscript addressing even one of these two issues would provide a paper that is of interest to the broader community.

Reply: as already stated in the ‘replies to comments of reviewer #1’, we will elaborate on the methodology and take the reviewer’s comment on board to focus on a more thorough theoretical investigation or justification of this approach.

Specific comments:

1. I don’t feel that the experimental setup is rigorous enough to justify the types of conclusions that are made. A six year model simulation (forced by a 30 year model SST climatology - from which years?) is compared to satellite observations of soil moisture in 2004-2006. Even understanding that the observations are uncertain, why should this model climatology match this observation period? The conclusions made about month by month discrepancies are somewhat speculative because of this.

Reply: the general argument of the reviewer is correct and we will make a note of this in the revised version of the manuscript. However, we wish to note the following: the overall aim of the paper is to assess the acceptability of seasonal soil moisture simulations from CMs with respect to uncertain satellite observations (or indeed estimations); this will also be emphasised in the revised manuscript. In the CMs normal atmospheric conditions are applied, which means we needed to omit all periods of extreme conditions from the observation period that ranged from 2003 to 2006 (these are the years that are complete and overlapping for both satellite products). We omitted 2003, as this had been subject to a very dry summer (which is referred to in the manuscript), and so ended up with 2004-2006 that we believe is a fairly representative period of atmospheric conditions comparable to those used in the CMs. We agree with the reviewer that conclusions need to be drawn with care and thus we will revise them in the new

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version of our manuscript.

2. There are many uncertainties which are not explicitly addressed. These should at least be dealt with qualitatively - what is the likely effect of not considering uncertainties associated with (in no particular order): - dependence of the two models (both use MOSES); - the fact that the two remotely sensed products and the model all have a different top soil layer depth; - the model climatology will not have any knowledge (via initial states) of wetter/drier than average soils at the beginning of 2004; - defining observational uncertainty by using just two products - isn't this under-sampled if this is our definition of uncertainty.

Reply: we agree that there are many uncertainties which are not explicitly addressed and therefore we will follow the reviewer's suggestion and address some of them qualitatively. For comments on the soil layer issue, please refer to the 'replies to comments of reviewer #1' document. We wish to note however that we do not believe that we have necessarily gone for an 'under-sampling' of the uncertainty. Given the capability to simulate soil moisture of the CMs used (as for instance, no knowledge in models about the actual initial soil conditions) and the simplicity with which hydrological processes are reproduced in these LSSs, we think that our acceptability assessment scheme is relatively fair and tries to minimise model penalisation.

3. Many of the conclusions drawn are speculative and sometimes not even investigated in the manuscript. For example, the abstract and conclusion suggest "Our work indicates that a higher resolution LAM has more benefits to soil moisture prediction than are due to the resolution alone and can be attributed to an overall intensification of the hydrological cycle relative to the GCM." The only investigation of the increased performance of the LAM was on page 2741: "In HadRM, processes are better discretized due to the higher resolution, which results in a more intense representation of the hydrological cycle (Jones et al., 1995). Therefore outputs of highly spatially varying parameters are more heterogeneous, which can lead to a better fit with spatially and temporally varying observations." I would argue this is a finding of Jones et al, not this

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paper.

Reply: in the revised version of the manuscript, we will focus more on the methodology and also add a figure or two to the result section that highlights more the advantage of a LAM over a GCM (see 'replies to comments of reviewer #1'). The findings of Jones et al. (1995) support our results but the reviewer is correct by implicitly suggesting that we should probably remove Jones' suggestion from the abstract. We will also rephrase our conclusions more carefully in the revised version.

4. I was a little confused about what fractional soil moisture. At times it was explicitly stated that the range 0-1 represented zero moisture to saturation (and this appears to be backed up by Figure 1), but at others I felt the authors implied this range reflected actual soil moisture content. How was saturation defined? How was it ensured that the spatial variation of saturation values in the remotely sensed algorithm matched model saturation?

Reply: by 'fractional soil moisture' we mean 're-scaled soil moisture' and apologise for any confusion and will ensure clarity throughout the revised manuscript.

Technical issues:

Generally I found the quality of presentation (figures, written language) to be very good.

1. The sentence starting line 19 on page 2734 does not make sense to me (grammar).

Reply: the intention was to simply highlight the control of soil moisture on heat fluxes and therefore its influence on weather. This will be clarified in the new version.

2. Line 18 page 2734: latent heat flux and ground heat flux are separate entities; a reader might think it is implied that they are equivalent here.

Reply: as this may in fact be misleading, we will rephrase this sentence.

3. Line 1 page 2735 "Cornwell" is repeated.

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Reply: this will be removed.

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