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Hydrology and Earth System Sciences Discussions

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

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 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.

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.

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.

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 paper.

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?

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2733, 2009.