

## ***Interactive comment on “Evaluation of the JULES land surface model in simulating catchment hydrology in Southern Africa” by N. C. MacKellar et al.***

### **Anonymous Referee #1**

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The paper presents a potentially interesting evaluation of the hydrological model part of a coupled land surface model. However, there are a number of problems with the paper.

The introduction does not take into account recent literature on hydrological modelling in general (PUB, uncertainty, models for hypothesis testing, sensitivity analysis, etc.) of which there is a lot, nor does it take into account the hydrological modelling literature on the specific region and catchments included in the paper. This is a serious issue given the generally poor results obtained from this study compared to much better results for other models.

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On page 5 starting at line 8 reference is made to the processes explicitly simulated but does not mention subsurface runoff or GW recharge (these are only mentioned later). There are also many other aspects of the models that could be inconsistent with the hydrological processes that are dominant in some of the catchments used. Deep groundwater conditions do not seem to be modelled and the models rely on intersection of the GW with the soils. The models do not seem to account for surface runoff that is driven by rainfall amounts only (rather than wetted soils through saturation excess). The latter will be a problem for the Orange River, as the authors appear to have discovered.

I found that the model descriptions were generally inadequate given that the model is being evaluated in the context of likely real hydrological responses.

P8, L14: There appears to be an implication here and later (in the discussion) of the results that the MOD16 data will not be biased and therefore the model outputs can be assessed relative to the MOD16 data. I do not think that there is any evidence that can be presented to confirm this and in fact the MOD16 data might be totally wrong (as we have found in other parts of southern Africa). The other point is that if the JULES model gets the streamflow volume correct for the correct rainfall, then how can the evaporation be a long way off (i.e. the Jules model does close the water balance). Perhaps MOD16 is OK for Okavango and Zambezi but not for the Orange?

Some of the results for the Okavango seem to ignore some of the processes that are possibly active in large catchments with extensive floodplains. Why is there no mention of the extensive amount of previous hydrological modelling work that has been done (and published) on the Okavango? P15 refers to the lack of water balance studies in the Okavango headwaters - some of these have been done as part of previous modelling studies.

I would be interested to know where the authors expect to get reliable soils data from and how this will be integrated at the very large grid scales that they are using.

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I get the impression that the authors have applied a very limited conceptual view to the hydrological processes that are likely to be dominant in these catchments and that view has been clouded by the limitations of the two models selected for use. I would argue that this is one of the problems with adopting a very coarse grid scale type of modelling approach. If one looks at the structure of the TOPMODEL it is quite clear that it cannot work for many catchments in the drier and more mountainous areas of southern Africa simply because the dominant processes are not represented. Models that do represent deeper and slowly draining groundwater as well as infiltration excess type surface runoff (even simulated implicitly) have been frequently demonstrated to be at least reasonably successful. Unfortunately, the authors appear to have decided to ignore the 30+ years of modelling experience in the region.

Specific comments:

P4, L12: What is meant by an 'unstable' regime? Do the authors mean 'variable'?  
P4, L14: I am not convinced about the existence of thresholds and the authors do not support this statement with any literature or other evidence.  
P4, L25: It would be a good idea if the authors look at recent literature (by Beven and others) on the use of models for hypothesis testing.  
P7, L9: 10% of what?  
P8, L10: Please specify if this is actual evapotranspiration.  
P10, L3: What is meant by mean climatologies when the graphs show rainfall and streamflow (these are not climatologies).  
P12, L5: Grammatical error - repetition.  
References: There are some page numbers missing from the references and these are not replaced with doi numbers.

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