Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-35-RC1, 2016

Geoscientific Model Development Discussions

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Interactive comment

Interactive comment on "The impact on the surface climatology from changing the land surface scheme in the ACCESS(v1.0/1.1) climate model" *by* Eva A. Kowalczyk et al.

Anonymous Referee #1

Received and published: 1 April 2016

General comments:

This paper try to analyze the impact of changing the MOSES land surface scheme by the CABLE land surface model in the ACCESS climate model. The introduction is very short and any references/discussion about land/atmosphere coupling processes (c.f. Betts et al. 1996; Betts 2009) were done. Only 2 references on previous works analyzing land surface-atmosphere interactions were done without any discussion. In the section 2, differences between the both land surface modules are relatively well described even if some details are missing. The section 3 presents the experimental design of the study. There is only two sentences that present the observations used to evaluate the model, that underline the poor scientific quality of this study. ERA Interim

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reanalysis alone cannot be used as observations. There is many “real” observations available to evaluate the model. The section 4 presents the main results. The text is very descriptive and generally boring even if the fact to use off-line runs to explain some in-line behaviours is a very good idea. Any tests of significance is done for all differences model versus observations and model versus model shown in this manuscript. The conclusion is well written and brings into focus the qualities and the defaults of this study. After having hesitated for a long time between rejected this paper or reconsidered it after major revisions, I think that this paper must be largely improved and it deserves a chance.

Specific comments:

P.1 - 2 : The introduction did not give the readers a sense of the state of the art, e.g., what are the land/atmosphere coupling processes, and which ones are important or not ? Did anyone else in the community attempt to analyze land/atmosphere coupling for global climate simulations? Only one sentence to sum up the work of Koster (2004) or Seneviratne et al. (2010) is not enough.

P.1, l.16 : I am not sure that LSM is a “key” component of a climate model, it is an important component but the key component is the atmospheric model (or the oceanic model).

P.3, l.25 : What is the value for Cs or how it is computed?

P.3, l.27 : What is the value for fr or how it is computed ?

P.4, l.19 : What is the value for c in the exponential or how it is computed ?

P.4, l.20 and l.30: Are you sure that CABLE “has a more complex representation” of canopy or displacement height than many LSMs? If so, prove it and explain the main differences with “many other LSMs”.

P.5, l.17 : “MOSES does account for this heat exchange.” These sentences are not clear to me.

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P.5, l.31 : The description of snow modules are very short while in the paper you access that “Warmer winter temperatures simulated by ACCESS1.1 over the snow covered areas of mid and high latitudes are attributed to differences in the snow parameterization in CABLE compared with MOSES.” Please provide more details. Does CABLE “has a more complex representation” of snow processes than many other LSMs ?

P.6, l.27 : LAI data are not the same for MOSES and CABLE. How you can be sure that most of the impacts in summer are not due directly to this difference rather than in the differences in land surface physics ? This fact must be addressed in order to improve the scientific quality of this paper. Normally, in this paper, the same LAI should be used in both models, perhaps by introducing an intermediate simulation (MOSES with LAI from CABLE or inversely).

P.7, l. : This is the most important negative point of this work. Any “real” observation was used. For continental precipitation, there is product : GPCC (certainly the best), GPCP, TRIMM (over tropics), etc. . . Try to use these products in your comparison. The same is true for temperature or cloud: CRU data gives Tasmin/Tasmax/Tasmean that allow to evaluate the diurnal cycle ; CERES, MODIS, etc. . . for cloud (see Pincus et al. 2012)

P.8 and after : The result and figure part are too descriptive and not enough scientific. Tests of significance (for example T-Test) must be done on the differences model vs obs as well as for model vs model. On figure, generally, the pattern where the differences are significant is shown with dotted panel. Without that, this article can not be accepted. Please only comment where the differences are significant statistically.

References :

Betts, A. K., J. H. Ball, A. C. M. Beljaars, M. J. Miller, and P. A. Viterbo (1996), The land surface-atmosphere interaction: A review based on observational and global modeling perspectives, J. Geophys. Res., 101(D3), 7209–7225, doi:10.1029/95JD02135.

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Pincus, R., S. Platnick, S. A. Ackerman, R. S. Hemler, and R. J. P. Hofmann, 2012: Reconciling simulated and observed views of clouds: MODIS, ISCCP, and the limits of instrument simulators. J. Climate, 25, 4699–4720

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