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Geoscientific Model Development Discussions

Interactive comment on "The CSIRO Mk3L climate system model v1.0 coupled to the CABLE land surface scheme v1.4b: evaluation of the control climatology" *by* J. Mao et al.

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This paper is an evaluation of the CSIRO Mk3L climate system model, a reduced-resolution coupled general circulation model. The focus of the paper is the assessment of the impact of replacing a simple land surface scheme with the more advanced CABLE land surface model. This is a relatively straightforward paper. The main conclusion is that the introduction of the more sophisticated CABLE land surface model has not degraded the climate simulation and has significantly increased the utility of the model, particularly for carbon-cycle related studies.

We thank the referee for his/her positive and constructive comments, which have enabled us to improve the manuscript considerably.

Minor comments:

1. Is there a reason why results from the simple K91 land surface scheme are not shown in Figs 2 and 3 and Table 2. Perhaps it cannot be run "offline". If that is the case, then it would be worth explaining this so that the reader doesn't wonder why the results weren't shown. If it can be run offline, it would be reasonable and informative to include the K91 results. Hopefully there would be a clear improvement between K91 and CABLE.

K91 is embedded within Mk3L and has not been configured to run offline. We have added a statement to this effect (Section 2.1).

2. In the analysis of the performance of CABLE (offline) against the six tower sites, there are several qualitative statements that are not well backed up. For example, the authors note that Harvard Forest is simulated poorly and that it may be due to poor prescription of leaf area index at that site and that it could be fixed trivially using site-specific LAI data. Have the authors tried this fix and shown that it worked? If not, I don't think it is appropriate to say that the poor simulation can be fixed trivially. Same thing for the Little Washita site. This statement about being easily resolved comes up again in the summary paragraph for this section on p. 1618.

We agree, and we have edited the text to resolve these issues (Section 3).

3. Similarly, the authors state that the Harvard forest site is well simulated for NEE, except with a 2 month lag. I think it is overstating things to say that this is well-simulated.

We agree, and we have edited the text to resolve this issue (Section 3).

4. Authors state on p. 1618 that the poor simulation of NEE probability density function in this model "highlights the distance land surface modeling still has to go to provide reliable estimates of this flux in a climate modeling system". This may be a correct statement, but generalizing to land surface models is not appropriate based on the evidence presented here. This statement should be made specific to CABLE or supported with a reference.

We agree, and we have edited the text to resolve this issue (Section 3).

5. P. 1620. "However, most also used flux adjustment to improve performance relative to observations." Most might be the wrong word. I thought it was many but not all models used flux adjustments in TAR.

We agree, and we have modified the text accordingly (Section 4.2).

6. There is an overemphasis on maps in their analysis of the model performance. A table summarizing global land RMSE and mean errors would be interesting and make the paper more quantitative.

We agree. We have added a new table (Table 3) which provides DJF, JJA and annual means, as well as the bias and root-mean-square error, for the two different versions of the model and for the following variables: near-surface air temperature, precipitation, net surface radiation and net primary productivity. We have also added a new paragraph at the end of Section 5 which discusses the data presented in the table.

7. P. 1623. Should note in the text that CASA is another model estimate of NPP.

We have revised the text accordingly (Section 5.2).

8. P. 1624. The comment that "state-of-the-art climate models . . . could not be routinely used for millennium-scale or multiple century-scale simulations" is not really correct, depending on how you define routinely. Obviously, multiple century-scale simulations are relatively easy and computationally possible (see the vast amount of simulations being submitted for CMIP5) for current generation GCMs. Millennial length simulations are also not out of the realm of possibility (CMIP5 protocol calls for a last millennium simulation that several groups have already performed). Authors should reconsider this statement and their motivation for the reduced resolution model. Certainly, there are applications for this type of model, but as stated it makes it seem as if this model's niche has already been superseded.

We agree that this comment does not accurately reflect the current state-of-the-art. The motivation for developing this model was to enable multi-millennial climate simulations and large ensembles, which would not be feasible with a typical CMIP5 model. We have revised the text accordingly (Section 6).

Technical comments:

1. Table 2, what averaging level was the RMSE calculated from – monthly, daily, hourly?

RMSE was calculated using 30-minute modelled and observed data. There is a statement to this effect in Section 3, and we have also added this information to the caption for Table 2.

2. Fig. 1 and Fig.2: Each individual figure on these plots is quite small. This is mainly a matter of taste, but I would recommend that the authors consider redrawing the figures

with the minimal amount of axes labels required (e.g., remove the x-axis label for all but the bottom figure and the y-axis) so that the actual plots can take up a larger fraction of the space.

We agree, and we have modified each figure accordingly.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1611, 2011.