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Geoscientific Model Development Discussions

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Interactive Comment

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.

Anonymous Referee #1

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General Comments

This paper describes a comparison with respect to land of control simulations of the CSIRO Mk3L atmosphere model when the model is run with two different land surface schemes: K91 and CABLE. The CABLE scheme is a replacement for the K91 scheme that couples carbon fluxes to the energy and water budgets, among other changes. The control simulations are evaluated against a range of reference datasets (observation and model derived) that span different periods of the twentieth century. The simulations are also compared with each other to assess the effect on the coupled land-atmosphere system of replacing K91 with the more sophisticated CABLE scheme.

This is a useful study because it provides a relatively clean benchmark of basic model variables in two models that may be compared against subsequent CABLE developments or results from other climate models. The authors conclude correctly that the change in land surface scheme from K91 to CABLE produces broadly similar coupled land-atmosphere simulations while adding important functionality to the Mk3L model.

The manuscript is well written, is clear in its intent and the authors achieve an honest balance in reporting where the models do well and do poorly. The figures are clear and clearly explained. However, given that this is a benchmarking article rather than a model description one, the analysis is limited in places and excludes important land surface variables. I recommend publication subject to the authors addressing the following comments.

Specific Comments

(1) The sites selected for the offline comparisons are (with one exception) all in North America or Europe, with none in the tropics. This seems an odd selection given that the most interesting model biases and differences occur in Eurasia and tropical South America. I can't help thinking that Fig 1 plots for a site in, say, Amazonia would be useful context for the results in later sections. Could the authors comment on how these sites were selected?

(2) The authors highlight (correctly) net radiation as being the most prominent difference between models in these simulations, particularly in Eurasia. They attribute this to increased insolation and albedo change (P1622, L15), but provide no evidence to support this. They also attribute the increase in Eurasian JJA sensible heat flux to the net radiation increase, but Figure 7 indicates that there is a coincident reduction in latent heat flux. This suggests a more complex and interesting picture involving a large scale reduction in soil moisture and possibly differences in snow melt timing, which in turn may contribute to the original insolation increase. The reader is also given no indication about the source of the albedo changes: are they from different prescribed

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ancillary fields (e.g., soil and vegetation) or from prognostic differences (e.g., snow cover)? While this study does not need an analysis of boundary layer and convection changes it does need a more thorough and quantitative description of differences at the land surface.

(3) Even without the above comments on the Eurasian energy budget I'd consider soil moisture and snow mass part of the basic set of variables that should be reported. I suspect that many of the main differences between K91 and CABLE simulations are affected by these variables, so it would be useful to see them. It's also rare to have the opportunity to compare so easily soil moisture between different land schemes (the authors note that the same soil layer configurations were used in both models).

(4) Could the authors comment on why they chose to compare their simulations with the IPCC TAR (2001) models rather than the more recent IPCC AR4 (2007) models?

(5) P1625: Discussion comments appear to contradict each other stating the net radiation change is "probably an improvement in CABLE" (L11) but note that there is an "absence of independent observations" (L15). How do the authors deduce the former given the latter? In fact, given that this is a relatively short manuscript, there are a few too many vague performance statements (e.g., "likely related to... leaf area", "zonal estimates are likely reasonable", "simulations are good") These add little that the reader can't see for themselves in the plots or are just speculation.

(6) The manuscript would benefit from a table of "bottom line" values for global, annual mean fluxes (LE, H, NPP) along with some discussion of how these compare with other estimates (observation and/or model derived). Benchmarks are most useful when they are specific. While the maps are enlightening in the context of this paper, it is very difficult in practice to make useful comparisons with similar plots from other models.

Technical Comments

P1614 S2: While the authors are right not reproduce the model description details

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of Phipps et al, a simple statement of the Mk3L grid resolution used (horizontal and vertical) would be useful orientation for the reader.

P1615 S2: The manuscript should include some brief description of differences in K91 and CABLE ancillary data where they are relevant to subsequent results and discussion. E.g., are these data derived from different sources for each model?

P1616 L15: Are offline simulations really forced with net radiation?

P1617 L25: "...there is clear skill in the upper tails...". This skill may be present, but it's difficult to tell from the plots alone. The percentage overlap metric refers to whole histograms, so it is presumably insensitive to large relative but small absolute differences in the tails? Would the skill be quantified better by some metric derived from upper percentiles?

P1618: The authors should comment (here or in the results) on how a simulation using pre-industrial atmospheric CO_2 but late twentieth century SSTs (which include 100+ years of warming) might affect their comparison with observed late twentieth century datasets.

P1620: The sections "Surface forcing fields" and "Surface fields" are structured oddly. The former heading is misleading because these are coupled land-atmosphere simulations rather than offline simulations of K91 and CABLE, so these variables are not strictly forcing the land schemes. Also net radiation results are described in both sections, which makes the distinction of the two sections confusing. Perhaps it would be clearer to move the net radiation results entirely to the second section and rename the sections simply "Temperature and precipitation" and "Surface energy budget".

P1621 L17: Figure 8.3 rather than 7.3 of the TAR, no?

Figures: Global maps of land-only variables are better presented when they run [-180, 180] degrees longitude rather than [0, 360] degrees, as this emphasises land rather than the blank Pacific. This is, I admit, a matter of personal taste.

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