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Geoscientific
Model Development
Discussions

Interactive comment on "The CSIRO Mk3L climate system model version 1.0 – Part 1: Description and evaluation" *by* S. J. Phipps et al.

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This paper describes the Mk3l climate system model, and gives some evaluation of the model performances for the pre-industrial climate. The paper is clear and nicely written.

1 - The model description is a good compromise between the need to be comprehensive, and the need to be concise. Full model description is given in the model documentation. I appreciate that this documentation is attached to the paper. Anyway, I didn't review these 237 pages with care.

We would first like to thank the referee for his/her positive and constructive comments,

which have helped us to improve the manuscript significantly.

2 - The paper then presents an evaluation of the mean state and the seasonal cycle of the atmospheric part (forced by SST), and of the oceanic part (forced by flux and surface restoring). An evaluation of the variability in coupled mode is given.

This way of performing the model evaluation is not relevant with the paper title (". . . climate system model . . ."). The evaluation of the model in coupled mode should be given. With the flux adjustments, one may guess that the model climatologies in forced and coupled mode are close. But its only a guess, because the paper gives no indication about that. And how close are forced and coupled modes ? Author cites Sausen et al. (1988) as unique reference for the flux adjustment method. This 1988 paper states: "The difficulty with the fully coupled approach is that the differences between the equilibrium climates computed in the decoupled and coupled modes are generally too large to apply linearized theory". Due to these non-linearity, there probably is a difference between forced model components and the full climate system model, and this should be perfectly clear in such a paper. If there is a difference, coupled climatology should be shown instead of forced ones.

As presently organized, the paper is then not an evaluation of the "climate system model". That's my main concern about this paper.

We agree. We have re-worked Sections 4 to 6 (now Sections 4 to 7) of the manuscript so that the evaluation of the model climatology is based upon the coupled climate system model simulation. As the coupled climate system model exhibits relatively little drift, this has made little difference to our results or conclusions.

3 - The paper presents the model variability without any word about the impact of the flux adjustments method. Several papers have addressed this issue, for instance :

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Brown, J., M. Collins, A. W. Tudhope, and T. Toniazzo, 2008: Modelling mid-Holocene tropical climate and ENSO variability: towards constraining predictions of future change with palaeo-data. Climate Dynamics, 30, 19-36. Kitoh, A., T. Motoi, and S. Murakami, 2007: El Nino-Southern Oscillation simulation at 6000 years before present with the MRI-CGCM2.3: Effect of flux adjustment. Journal of Climate, 20, 2484-2499.

This subject should be addressed, with relevant references.

We have revised the manuscript to incorporate a discussion of the potential impacts of flux adjustments on the simulated climate variability, including relevant references (Section 6).

4 - The model is designed for palaeoclimate research. I clearly doubt that a model with flux adjustments can be used for different climates. Even for climate which may seems no so different from pre-industrial : Holocene is characterized by large changes of the seasonal cycle (amplitude and phase), particularly in the monsoon region. Flux adjustments will prevent any change of season length, phase shift, etc . . . You cannot hide this strong limitation, and maybe you should explain what kind of palaoeclimate research could be done with this model (and which could not).

The flux adjustments applied within Mk3L consist of a fixed annual cycle. We have added statements which clarify this point (Sections 2.5, 4.4, 6). There is no reason to believe that fixed annual-cycle adjustments would act to prevent the model from simulating changes in the seasonal cycle. Indeed, in Part 2, it will be shown that the model is capable of simulating changes in the seasonal cycle over the Holocene.

However, we do acknowledge the point made by Cubasch et al. (1992) that coupled models that employ flux adjustments should only be regarded as being suitable for

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simulating small perturbations about the reference state. We have added a statement to this effect (Section 2.5).

Other major comments

Part 2.5 The model resolves the diurnal cycle. The text states that this improves the simulated climate in the tropics. However, papers cited use a vertical resolution of 10m in the upper ocean, and a TKE scheme for the vertical turbulence. Diurnal cycle in the upper ocean is strongly linked to these features. Is this sensitivity observed in coupled models with a 25m vertical resolution in the ocean, and a fixed vertical diffusivity? It seems that the model has no penetrative solar radiation, which can also impact the diurnal cycle representation.

The model simulates a diurnal cycle of $\sim$1K in tropical SSTs. However, the referee is correct in stating that the model does not allow for penetrative solar radiation, and we have added a statement to this effect (Section 2.4).

The flux adjustments should be a part of the model evaluation. Please give a figure showing flux adjustments, for each field. Is the adjustment in annual mean, seasonal, . . . Please describe how the flux adjustments are computed.

We have added a new section which explains the nature of the flux adjustments and describes how they are derived (Section 4.4). A new figure has been added, which shows the adjustments for each field (Figure 3).

** Minor comments **

About the Model documentation. A clickable PDF would we appreciable (links in table of contents, bibliographic references, figure references, etc . . .)

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We will implement this in future versions of the documentation.

As the model "is freely available to the research community" (part 8, line 4), the licence type (GPL, LGPL, other) should be mentioned.

The licence is a bespoke licence which restricts use of the model to non-commercial research purposes only. We have revised the manuscript to include this information (Sections 1, 3).

Part 2.1 The radiation is computed every 2 hours. That means that the "model time step" is 2 hours, not 20 mn, even though some model processes are computed more often, because all processes are updated every 2 hours. What is the "20 mn" time step given line 16: physics time step, are also dynamics ?

The model completes the main timestepping loop once every 20 minutes, and thus the model timestep is most accurately described as being 20 minutes. *Full* radiation calculations are only conducted once every six timesteps (i.e. once every 2 hours). However, the upward longwave and downward shortwave fluxes at the surface are re-calculated every timestep, in order to smooth the diurnal cycle of net radiation. We have revised the manuscript to make these points clear (Section 2.1).

Part 2.1 The transport scheme is a semi-lagrangian. What are its properties of conservation?

The semi-Lagrangian moisture transport scheme is fully conservative. After each leapfrog timestep, the moisture field is processed: any negative values are removed, and a uniform offset is applied to ensure that the global integral is conserved. We have added a comment to this effect (Section 2.1).

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Part 2.4 Tracer diffusivity is isoneutral. Please specify whether there is an horizontal background diffusivity or not.

There is no background horizontal diffusivity. We have added a comment to this effect (Section 2.4).

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