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Interactive
Comment

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 is a clear and concise manuscript which thoroughly describes a low resolution coupled AO-GCM. I found the manuscript easy to follow, the model well described and the evaluation well laid out.

We would first like to thank Chris Jones for his positive and constructive comments, which have allowed us to improve the manuscript significantly. We also thank him for valuable suggestions regarding future development of the model.

As an overall comment, we note that the current manuscript is intended to be the first half of a two-part manuscript. Part 1 describes the model and evaluates the control state, while Part 2 will evaluate the transient and equilibrium response of the model to external forcings. Part 2 will be submitted for publication very shortly.

As a general comment, I would have liked to see some discussion of how this model's sensitivity compares with the parent GCM. The evaluation of the steady state shows good performance of the low-resolution model. But how does its climate response (transient and/or equilibrium) compare with the high-resolution version? It is a key question to know have the changes required to enable a low resolution version affected the CHANGES you would see in a climate change experiment. See, for example, Jones et al (2005, Clim. Dyn.) on the tuning of the FAMOUS low-resolution model to see that the climate sensitivity can vary markedly during the tuning/calibration phase.

There is no single parent model, as the atmospheric component of Mk3L is taken from the CSIRO Mk3 climate model, while the oceanic component is taken from the CSIRO Mk2 climate model. Unfortunately, this precludes any simple comparison. However, the current manuscript does compare features of Mk3L against Mk2 and Mk3, as appropriate.

We will ensure that Part 2 of this manuscript carefully compares the sensitivity of Mk3L against the sensitivities of Mk2 and Mk3.

I also wonder why you choose to do a pre-industrial simulation and evaluate against present day observations. You note in several places that the comparison may not be valid due to the different periods. So why not do a present day simulation and compare with present day observations? Clearly this isn't perfect either as the real presentday state is not in steady state with the forcing, but this will be a smaller error than a pre-industrial vs present day comparison. Especially when evaluating the atmosphere model with prescribed present day SSTs (as per an AMIP experiment) then this should

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make a very good comparison... In fact, given the speed of the model you could run the full 20th century in 2 days and do a really nice model-obs evaluation... This would strengthen your comparison considerably.

We acknowledge that there is no unambiguously "right" or "wrong" way of evaluating a model of this nature. While the use of a pre-industrial control state does introduce an inconsistency with observations, the use of a 20th century simulation for evaluation purposes would introduce other complications, such as the use of a non-steady state of the climate system as the basis for the analysis.

As Mk3L is intended primarily for palaeoclimate research, we note that the current manuscript serves two purposes: to characterise the baseline behaviour of the model on timescales ranging from annual to millennial, whilst also evaluating the model against observations. The use of a 20th century simulation as the basis for the evaluation would preclude the first of these aims. We have revised the manuscript to make the intention of the analysis clearer (Abstract, Sections 1, 5, 6).

We also note that the use of a pre-industrial control state is mandated by model intercomparison projects such as PMIP. By adopting this experimental design, the control state of the model can be directly compared against that of other models, thereby increasing the value of the manuscript. We have added a comment to this effect (Section 4).

We further note that Part 2 of this manuscript will include a 20th century simulation.

I would also like a bit more in-depth discussion/opinion on some of the things you find. The presentation of the "what" is well done, but you could add more of the "so what". e.g you have a temperature drift of 0.015 K/century, which sounds small, but a sea-ice *drift of +30% in the southern hemisphere. What does this mean for a user? What would this mean if you wanted to run a glacial-inter glacial transition? Would it be serious?*

We have added additional comments which expand upon the limitations imposed by the drift (Sections 7.5, 9).

I have only read the supplementary material briefly, but found no problems with it. As far as I could tell any questions I could think of I would have been able to find an answer. I recommend publication after addressing these and a few more minor issues listed below.

Chris Jones.

1. p.221, lines 2-5. Strictly it would be better to cite CMIP3 and CMIP2 activities, rather than IPCC AR4 and TAR reports.

We have added references to CMIP2 and CMIP3 (Section 1).

2. p.221. Would be useful to explicitly name the models you compare to here – e.g. for the Hadley Centre GCM, the high-resolution parent model is HadCM3, the lowresolution version is FAMOUS. (Note, better to cite Jones et al., 2005, Clim. Dyn. which is the published peer-reviewed version of the technical note you cite as Jones et al. 2004).

We have revised the manuscript accordingly (Section 1).

3. p.225, line 12. Here, and occasionally at other points you mention observed quantities (here sea-ice). But these won't be available for you target simulation periods?

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So can you clarify how you spin-up palaeo climate simulations without the required observed SSTs/sea-ice?

SST and sea ice datasets are only required to spin up the atmosphere-land-sea ice and ocean models. For palaeoclimate simulations, there is no need to spin up the model; rather, the simulations would branch off a pre-industrial control simulation. Mk3L is sufficiently fast that it is feasible to integrate it for the timescales required to bring the deep ocean into thermal equilibrium for palaeoclimate scenarios.

4. p.226, line 1. do you really have a dynamic vegetation model here? (in which case some evaluation of the simulated vegetation is required). Or by "seasonally varying vegetation fractions" do you mean leaf-area-index?

No, the vegetation model is static. However, the vegetation fraction can be reduced when snow cover is present, to allow for vegetation being partially or completely buried beneath snow. We have revised the manuscript to make this clearer (Section 2.2).

5. p.229. Line 17-22. can you explain why coupled and offline runs require different ocean timestepping? Is this a time-saving measure? Why need 20 minutes in standalone but 1 hour coupled?

This is purely a time-saving measure. A momentum timestep of 20 minutes allows a longer tracer timestep of 1 day to be used. As the tracer timestep determines the execution time of the model, this combination gives the fastest runtime performance. We have added comments to this effect (Section 2.4).

6. p.231, lines 15-21. you list the quantities passed from atmos to ocean. What about radiation. Is that included somehow in your heatflux? Is there any penetration of light into the surface ocean?

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Radiation is indeed included in the surface heat flux. The model does not account for the effects of penetrative solar radiation. We have added a statement to this effect (Section 2.4).

7. p.231, lines 25-27. It would help me (a non-expert in various techniques of flux adjustments) to explain more fully what you mean by the term flux-adjustments. My previous use of these has involved a two step process of relaxing the ocean SSTs and salinity to a climatology, and then diagnosing the fluxes required to do this and to apply these subsequently to counter any climate drift. It seems here your technique is subtly different, involving corrections to both fluxes and state variables – is that right?

The flux adjustments are derived in the same way that you describe, although corrections to the state variables are also derived by comparing the simulated SSTs and SSSs with the climatology used to force the model. We have added a new section, which explains the procedure used to derive the flux adjustments (Section 4.4).

8. p.233, line 26. You quote 60% and 85% of earth surface agreeing with obs. But given you prescribe SSTs and they cover 2/3 of the world is this really a good fraction? Can you rather quote the fraction of land/ocean area that agrees (i.e. mask out the areas you fix!)

The evaluation of the mean climate is now based upon a coupled climate system model simulation with dynamic SSTs, in response to comments by Referee #1 (Sections 4, 5).

9. p.235. Line 18. You mention pre-industrial GHGs – what other GHGs than CO2 do you use? Can you list all of the input radiative forcings this model requires? What about aerosols? And other natural forcings (volcanoes, solar, orbital??)

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The radiation scheme in Mk3L allows for the radiative effects of CO_2 and ozone. The model does not explicitly allow for the radiative effects of any other anthropogenic greenhouse gases. The solar constant and the epoch (which is used the calculate the Earth's orbital parameters) can also be varied by the user. This is documented in the Supplementary Material. We have also added statements to this effect to the manuscript (Section 2.1).

10. p.235. Line 20. you discuss problems are particularly bad near the tropopause. How do you define the tropopause here? We found problems with FAMOUS because the prescribed ozone concentration was not well enough resolved vertically to capture the tropopause and we would occasionally get stratospheric ozone concentrations in the troposphere. You might want to check how you prescribe ozone around the tropopause level – as this may be a source of your errors.

In this analysis, the tropopause is defined simply as the location of the temperature minimum between the troposphere and the stratosphere. We will check the prescribed ozone concentrations, which are taken from the AMIP II recommended dataset.

11. p.237. When discussing sea-ice can you give a bit more discussion? You dedicate 4 figures to sea-ice so you should dedicate more text I think. e.g. you appear to have a too small seasonal cycle of NH extent, but too big seasonal cycle in ice volume. Is this realistic? Does it mean you preferentially grow ice downwards instead of outwards?

We have expanded upon the analysis of the simulated sea ice, with particular regard to any areas of disagreement between the model and observations (Section 5.2).

12. section 5. can you describe what aspects of the World Ocean Atlas temperatures you use? What date? Do you impose a seasonal cycle? What time resolution? Don't assume the reader is familiar with what's in this dataset.

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The annual-mean temperatures and salinities are used to initialise the model, while the seasonal cycles of SST and SSS are used to spin it up. We have added comments to this effect, as well as adding a brief description of the World Ocean Atlas (Section 4.2).

13. sec.6. You run for 4000 years and then assess the years 200-1200. Why not use the full run? Why analyse so close to the beginning?

The first 200 years of the simulation represent a spin-up period. We decided to base the analysis upon the following 1000 years, so that it is not excessively affected by the long-term secular drift. We have revised the manuscript to make this clear (Section 4.3).

14. p.242. You're right that spatial resolution makes it harder to simulate dynamical features such as ENSO. Can you comment on implications of this? e.g. does it limit the uses of such a model? It may not be an important feature of millennial scale simulations, but you couldn't use this model for seasonal forecasting for example... what other restrictions should a potential model user know about?

The model has considerable skill at capturing the larger-scale features of the climate system but, because of the reduced spatial resolution, is less successful at the regional scale. It would therefore be of limited utility for studying problems that require resolution of processes at a fine spatial scale, or for studying problems that require a realistic simulation of interannual variability. We have added comments to this effect (Section 9).

|it 15. p.244, sec 6.2.2. the southern hemisphere sea ice expands by about 30% - this sounds relatively serious. Can you expand on why this is and comment on whether this

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prevents the use of the model on any timescales? Is this just a spin-up issue? Have you tried asynchronous spin-up techniques?

We have not tried asynchronous spin-up techniques, but we will do so in future. The drift is a consequence of the spin-up procedure, and the reasons for this are discussed in Section 7.5. We have also expanded upon the discussion of the limitations that the drift imposes upon the utility of the model (Section 7.5).

16. sec 6.2.4. Is this trend in ACC linked to the sea-ice? Can you comment maybe if one might drive the other?

Analysis of transient climate change simulations conducted using this model (not yet published) and the CSIRO Mk2 climate model (Bi et al., 2002) show that the strength of the ACC is sensitive to changes in the density structure of the ocean interior. It is likely that this mechanism explains the ACC trend here, although we have not verified this. We have added a comment to this effect (Section 7.4).

17. sec 6.2.5. summary – last line. When you say "more realistic sea ice" - I think you mean "more fully spun-up"? Or do you really mean that your initial state is not close to reality?

The latter point is correct. The atmosphere-land-sea ice model spin-up run had deficient sea ice cover in the Northern Hemisphere. Upon coupling to the ocean model, there was a large initial adjustment, during which the sea ice cover in the Northern Hemisphere expanded. It has been shown (Phipps, 2006b) that this change gives rise to the ongoing cooling trend within the coupled model. We have revised the manuscript to make this point clearer (Section 7.5).

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18. p.247, line 17. you say flux corrections are "inherently undesirable" - what happens when you run without them? Have you tried developing a non-flux adjusted version? When we developed FAMOUS we found a simple change to the north Atlantic bathymetry (removing Iceland) was sufficient to allow better northward heat transport and allow us to run without flux corrections. You may find if you try it that only relatively small changes are required for Mk3L also.

When this version of the model is run without flux adjustments, the rate of drift is unacceptably large. In particular, the thermohaline circulation collapses after less than 100 years. We have added a comment to this effect (Section 4.4).

By increasing the horizontal spatial resolution of the ocean within Mk3L, we have been able to develop a version of the model that can be run without flux adjustments. This version of the model is being evaluated, and will form the basis of a subsequent manuscript.

19. p.247 last line, "whole new class of scientific questions"... such as what? Can you suggest what you would use such a model for?

The incorporation of additional processes – particularly dynamic vegetation and an interactive carbon cycle – would allow the representation of new feedbacks within the model. This would enable it to be used to study the role of these feedbacks within the climate system. Combined with a dynamic ice sheet, the model would be particularly suitable for studying the role of abrupt climate change. We have added comments to this effect (Section 8).

20. more generally, one key aspect of reduced resolution models is the concept of "traceability" - to enable not just science with the model, but to help guide use and development of the higher resolution counterpart. Can you comment on the relationship *between Mk3L and CSIRO state-of-the-art high res models (e.g. those being used in CMIP5)? Are they related? What are the differences in resolution, speed, process complexity, performance etc...*

We agree that "traceability" is highly desirable. This is somewhat complicated here by the fact that Mk3L does not have a single parent model. Nonetheless, the relationship and differences between Mk3L and the parent models are discussed in Sections 1 and 2.

21. I notice in figure 1 you have Iceland in the atmosphere model – but in the text you say you don't have it in the ocean model. How does the coupling deal with these mismatches between components?

Spatial interpolation is used to estimate values for locations where there are mismatches between the positions of the coastlines. Uniform offsets are then applied to ensure conservation of global integrals. We have added a paragraph which describes this (Section 2.5).

22. if palaeo runs are your motivation presumably you want to regularly change the models coast line/sea-level etc. Is this easily done?

Yes. In the case of both the atmosphere-land-sea ice and ocean models, the topographical information (including the positions of the coastlines) is specified within auxiliary data files that can be manipulated by the user. This is documented in the Supplementary Material.

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