**Geoscientific
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Interactive comment on "The CSIRO Mk3L climate system model version 1.0 – Part 2: Response to external forcings" *by* S. J. Phipps et al.

S. J. Phipps et al.

s.phipps@unsw.edu.au

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This is a well written and clear manuscript which documents the response of the CSIRO Mk3L model to various external climate forcings, from realistic, historical forcings that can be compared with observational evidence to stronger idealised forcing that can be compared with other climate models under similar scenarios. The paper is a follow-on from Part 1 which documented the model set-up itself and spin-up and control performance.

The paper clearly achieves its objectives of showing that Mk3L model broadly captures the key features of observed climate changes in response to various past external forcings, and also that it's future projections are likely to be compatible with other (much

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more computationally expensive) GCMs.

My only main recommendation would be to better understand how the model can simultaneously have a low TCR and a high ECS. Both values are shown to be within previous GCM ranges, but it seems strange to be at the high end in one and the low end in the other. The two are not independent and are generally related (see AR4 ch. 8) – so what are the causes and implications of this? We found with FAMOUS (Jones et al 2006) very strong sensitivity of ECS to tuning parameters – have you investigated if this is true with Mk3L too and whether this jointly affects TCR and ECS? It would be valuable to know the reasons behind what affects the sensitivity.

Aside from this I have just a few minor corrections/clarifications as listed below.

Chris Jones

We would first like to thank Chris Jones for his positive and constructive comments, which have helped us to improve the manuscript significantly. We also thank him for his helpful suggestions regarding future work.

With regard to the TCR and ECS of Mk3L, we have not yet performed parameter sensitivity experiments of the type suggested. However, an Honours student at the University of New South Wales will be conducting work of this nature during 2012.

In attempting to address this issue, we have first performed a more accurate calculation of the value of the TCR. The value of 1.5 K quoted in the original manuscript was based on the difference in global-mean surface air temperature between years 65–74 of experiment 4CO2 and the equivalent period of the control simulation. However, if we

base the calculation on years 61–80 instead, as in the IPCC AR4 report, we obtain a slightly larger value of 1.59±0.08 K.

We have then used the theoretical framework of Gregory and Forster (2008) to study the transient response of the model. We obtain a climate feedback parameter of $\alpha = 1.48 \pm 0.19 \, \mathrm{W \, m^{-2} K^{-1}}$, an ocean heat uptake efficiency of $\kappa = 0.64 \pm 0.17 \, \mathrm{W \, m^{-2} K^{-1}}$ and a climate resistance of $\rho = 2.12 \pm 0.14 \, \mathrm{W \, m^{-2} K^{-1}}$. The values of all three parameters are consistent with the medians of the 5–95% uncertainty ranges derived for the CMIP3 ensemble by Gregory and Forster (2008). Thus, from this perspective, the transient response of Mk3L is entirely consistent with that of other models. However, with $F_{2\times} = 3.7 \, \mathrm{W \, m^{-2}}$, the values of ρ and α derived for Mk3L equate to a TCR of 1.75±0.12 K and an ECS of 2.50±0.32 K, respectively. These values differ from the actual transient and equilibrium responses of the model. We therefore conclude that the relatively low value of the TCR is a consequence of the "cold start" effect, while the relatively high value of the ECS is a consequence of the time-dependence of climate feedbacks.

We have comprehensively revised the manuscript in light of these results.

Reference:

Gregory, J. M., and P. M. Forster (2008), Transient climate response estimated from radiative forcing and observed temperature changes, J. Geophys. Res., 113, D23105, doi:10.1029/2008JD010405.

1. Although technical details of the model are comprehensively covered in "Part 1" of this pair of papers, it might be useful for the reader to recap briefly in the intro on the relative timings/performance of Mk3L and how to obtain it for general use. (e.g.

something like "Mk3L can run 1000 model years in just X days on a XXX capability PC...")

We have revised the text accordingly.

2. it seems a shame that non-CO2 GHGs cannot be explicitly included and must be rolled into a single Co2-equivalent value (page 3370). Are there any plans to do so? Although probably valid for radiative forcing, treatment of CO2 and non-CO2 GHGs separately would allow one to explore non radiative effects of CO2 (such as on plant productivity or ocean acidification), but this would be hampered if other GHGs such as methane were included in the CO2 values used by the model. Although not yet a carbon-cycle GCM, can you comment if this is planned for a future version?

A new radiation scheme has been incorporated into the CSIRO Mk3.6 GCM (Rotstayn et al., 2012), to which Mk3L is closely related. This scheme allows for the direct representation of the radiative effects of methane, nitrous oxide and halocarbons, as well as carbon dioxide. CSIRO Mk3.6 also includes an interactive aerosol scheme.

It is planned to incorporate both of these schemes into Mk3L in future. They will complement the ocean biogeochemistry scheme of Matear et al. (2003) — which has already been incorporated into Mk3L — and transform it into a full Earth system model.

References:

Matear, R. J. and A. C. Hirst (2003), Long-term changes in dissolved oxygen concentrations in the ocean caused by protracted global warming, Global Biogeochem. Cy., 17, 1125, doi:10.1029/2002GB001997.

Rotstayn, L. D., S. J. Jeffrey, M. A. Collier, S. M. Dravitzki, A. C. Hirst, J. I. Syktus and K. K. Wong (2012), Aerosol-induced changes in summer rainfall and circulation in the Australasian region: a study using single-forcing climate simulations, Atmos. Chem. Phys. Discuss., 12, 5107-5188, doi:10.5194/acpd-12-5107-2012.

3. p. 3375 – why not increase the CO2 back to 280 during this period? Surely you want to finish at your pre-industrial forcing state?

The stated aim of this experiment is to examine the transient response of Mk3L to orbital forcing over this period. All the other boundary conditions on the model (including atmospheric greenhouse gases and solar irradiance) are therefore held constant. We acknowledge that a 3 ppm difference in the atmospheric CO_2 concentration relative to the control simulation is undesirable, but we note that this equates to a radiative forcing of only $0.06 \, \mathrm{W \, m^{-2}}$. We consider this to be negligible.

We have revised the text to clarify these issues.

4. In section 3, you present compelling arguments that the acceleration technique does not significantly affect the surface climate – but what about components with longer timescales? Presumably ocean heat content for example differs when a very large acceleration is used? Can you show how this differs and discuss how important this might be for analysis?

We have added a new subsection (Section 3.5 in the revised manuscript) and a new figure (Figure 11 in the revised manuscript) which consider the simulated ocean temperatures. We find that the use of acceleration does affect the temperatures in the ocean interior. While the differences between the experiments are small in magnitude,

this indicates that significant errors in the response of the model could arise if large acceleration factors were to be used to simulate very long periods of time.

5. Section 4 – Last Millennium. Can you outline if there are any differences between your experimental design and the CMIP5 protocol?

There are two minor differences between our experimental design and the CMIP5/PMIP3 protocols for the Last Millennium (850–1850 CE) and Historical (1850–2005+ CE) experiments. For consistency, we use the solar reconstruction of Steinhilber et al. (2009) for the entire period 1001–2000 CE, whereas the CMIP5/PMIP3 protocol specifies a transition to the reconstruction of Wang et al. (2005) after 1850 CE. We also use the Law Dome dataset of MacFarling Meure et al. (2006) to provide the concentrations of all anthropogenic greenhouse gases, whereas the dataset supplied by CMIP5/PMIP3 uses data from multiple ice cores to derive the N_2O concentration. Both these differences in the experimental design are negligible.

We have added a paragraph to this effect.

References:

MacFarling Meure, C., D. Etheridge, C. Trudinger, P. Steele, R. Langenfelds, T. van Ommen, A. Smith and J. Elkins (2006), Law Dome CO_2, CH_4 and N_2O ice core records extended to 2000 years BP, Geophys. Res. Lett., 33, L14810, doi:10.1029/2006GL026152.

Steinhilber, F., J. Beer and C. Fröhlich (2009), Total solar irradiance during the Holocene, Geophys. Res. Lett., 36, L19704, doi:10.1029/2009GL040142.

Wang, Y.-M., J. L. Lean and N. R. Sheeley, Jr. (2005), Modeling the Sun's magnetic field and irradiance since 1713, Astrophys. J., 625, 522-538.

6. p.3383 – you say anthropogenic aerosols can't be included – but you could fold these into your CO2e value as you have done with other GHGs? Clearly this wouldn't be precise, but better than neglecting them entirely. As you comment later, omitting them leads to too much warming.

We agree. However, we wished to employ consistent boundary conditions throughout the last millennium experiment, and the lack of any suitable reconstructions spanning the whole of this period was therefore a barrier. We note, in particular, that CMIP5/PMIP3 do not provide anthropogenic aerosol data prior to the year 1850 CE. Nonetheless, we will attempt to use the suggested approach to incorporate the effects of anthropogenic aerosols in future work.

We have added a comment to this effect.

7. p.3383, why use the control state to initialise the last millennium run when you have a transient 6k run – would this not be better suited?

We felt that a dedicated spin-up run, using boundary conditions appropriate for the year 1000 CE, would be more suitable. The transient simulations of the past 6ka only took into account the effects of orbital forcing, whereas the last millennium experiment also took into account changes in anthropogenic greenhouse gases, solar irradiance and volcanic emissions. As such, there would have been a discontinuity in the boundary conditions had one of these simulations been used to initialise the last millennium experiment.

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8. Is M2009 the best dataset for model evaluation over the instrumental period? How does it compare with other datasets?

M2009 is the most recent and most comprehensive hemispheric temperature reconstruction of which we were aware, as it incorporates 1209 different proxy records. We therefore felt that it was the most suitable dataset for the purposes of this comparison. The reconstructed difference in mean Northern Hemisphere surface air temperature between the Mediaeval Climate Anomaly (950–1250 CE) and the Little Ice Age (1400–1700 CE) is +0.24 K, which is consistent with other comparable reconstructions (e.g. Figure 6.10c of the IPCC AR4 WG1 report).

As the primary aim of this manuscript is to characterise the response of Mk3L to external forcings, we feel that an intercomparison of different reconstructions would be beyond the scope of this study.

9. p.3393 – you mention differences between methodology (slab model versus running coupled model to equilibrium) – but having a fast model like Mk3L would allow you to test techniques such as the Gregory method (2004, J. Clim: sudden 4xCO2 step change – see CMIP5 simulation protocol) for diagnosing ECS in a coupled model. You could asses how well this technique gets the final equilibrium sensitivity.

In light of the new analysis of the TCR and ECS, using the theoretical framework of Gregory and Forster (2008), we no longer attribute the relatively high value of the ECS to a difference in methodology.

Nonetheless, we agree that the suggested approach has merit and we will consider this in future work.

10. Sec 5.6 – can you explain the mechanism of changes in deep water formation? Are they driven by SST changes to stratification, or changes in freshwater input? This section should include discussion of recent work that shows how stability (and reversibility) of Atlantic overturning depends on freshwater transport into the basin. See Hawkins et al, (2011, GRL) and Drijfhout et al. (2010 Clim. Dyn.)

We have found that the evolution of the thermohaline circulation is driven by a combination of rapid hydrological changes in the atmosphere and slow thermal changes in the ocean. The initial response of the model to increasing CO_2 is an increase in poleward moisture transport, which increases freshwater input into the North Atlantic. This reduces the density of the surface waters and thus weakens the thermohaline circulation. As soon as the CO_2 concentration is stabilised, the poleward moisture transport stabilises too (except in experiment 4CO2, where a slow increase continues until around year 500). This allows the thermohaline circulation to begin to recover. In experiment 2CO2, this is sufficient for the circulation to recover to its original strength. Warming at depth in experiments 3CO2 and 4CO2 also acts to reduce the stratification of the water column, allowing the thermohaline circulation in these experiments to ultimately recover to its original strength as well.

We have added the results of this analysis to the manuscript and incorporated a new figure (Figure 23 in the revised manuscript). We have also included references to Drijfhout et al. (2011) and Hawkins et al. (2011) in the discussion.

References:

Drijfhout, S. S., S. L. Weber and E. van der Swaluw (2011), The stability of the MOC as diagnosed from model projections for pre-industrial, present and future climates, Clim. Dynam., 37, 1575-1586, doi:10.1007/s00382-010-0930-z.

Hawkins, W., R. S. Smith, L. C. Allison, J. M. Gregory, T. J. Woollings, H. Pohlmann and B. de Cuevas (2011), Bistability of the Atlantic overturning circulation in a global climate model and links to ocean freshwater transport, Geophys. Res. Lett., 38, L10605, doi:10.1029/2011GL047208.

11. There is a recommendation for IPCC AR5 draft chapters to use map projections beginning at 190W for land or 20E for ocean quantities. You might find this a good practice to adopt.

We agree that this would be a good practice, and we will adopt this in future work. However, we would prefer to retain the current projections in this manuscript so as to ensure consistency between Parts 1 and 2.

12. Fig 15 – can you order your panels consistent with other figures? (a,b on top, c, d below)

We have revised the figure accordingly.

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