



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

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The manuscript has a clear structure, is well written, and meets its goal to document the response of Mk3L to external forcings. It is well articulated with the part I already published in GMD. Its focus is perfectly suited for GMD.

The paper needs a few improvement about the 6ka BP section, and about the analysis of the millennium simulation. And some precisions about the significance of some indicators are needed.

We would first like to thank the referee for his/her positive and constructive comments, which have helped us to improve the manuscript significantly.

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*For the 6ka BP simulation, reference to Cheddadi (1997) are outdated. More recent reconstructions exist, for instance: Bartlein PJ, Harrison SP, Brewer S, Connor S, Davis BAS, Gajewski K, Guiot J, Harrison-Prentice TI, Henderson A, Peyron O, Prentice IC, Scholze M, Seppa H, Shuman B, Sugita S, Thompson RS, Viau AE, Williams J and Wu H (2011). Pollen-based continental climate reconstructions at 6 and 21 ka: a global synthesis. *Clim Dynam* 37(3-4):775-802 Brewer S, Guiot J and Torre F (2007). Mid-Holocene climate change in Europe: a data-model comparison. *Clim Past* 3(3):499-512*

We have retained one reference to Cheddadi et al. (1997) in the opening paragraph, where we list available reconstructions. However, we have added references to these more recent manuscripts as well. In the subsequent discussion, we have removed all references to Cheddadi et al. (1997), and we have replaced them with references to these more recent manuscripts instead.

The analysis of Fig. 13 states that “Mk3L captures the variations in NH SAT well”. I find this not convincing from the figure. You should be more specific on the time scale. For the short (decadal) time scale, a more qualitative may confirm your sentence, but there is nothing about this in the paper, except for the main eruptions. There is no reason that decadal variability is phased between model and data, but amplitude spectra could be discuss. Further in the text, the analysis of LIA and MCA is more convincing. However, these two periods are chosen because data tell us to do so. Are they suitable to analyze the model ? Would an independent analysis yield a different choice of periods to separate warm and cold periods? And please, give a confidence interval for all temperature changes. It is given for the Maunder minimum, but not for the 1645-1715 period.

From a statistical perspective, the only meaningful statement that can be made is whether or not the model lies within the confidence interval on the reconstruction. The

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ten-year running mean for the model lies within this interval for almost all of the pre-industrial period, and this is what we meant when we stated that it captures the variations in NH SAT “well”. We have revised the text to make this meaning clearer, and we have added an explicit statement that we are referring to decadal-to-centennial scale variations in the climate.

According to M2009 and other hemispheric temperature reconstructions (e.g. Figure 6.13d of the IPCC AR4 WG1 report), the MCA and LIA represent the pre-industrial extremes in centennial-scale NH SAT during the last millennium. As such, they represent the optimal test of the ability of the model to simulate centennial-scale changes in the climate, and it is for this reason that they were chosen. We have added a comment to the manuscript to clarify this. In regard to the identification of the MCA and LIA as warm and cold periods, and whether an alternative analysis might identify other periods instead, such an analysis would be of scientific interest but is beyond the scope of this manuscript.

Throughout the analysis of the last millennium experiment, we have also added 95% confidence intervals to all the quoted temperature changes.

Minor remarks

For the relevant experiments, could you be precise on they respect, or not, the different protocols (PMIP2/3, CMIP5) ?

The mid-Holocene experiment (Section 2) is consistent with the PMIP2/PMIP3 protocols.

There are two minor differences between the last millennium experiment (Section 4) and the CMIP5/PMIP3 protocols for the Last Millennium (850–1850 CE) and Histor-
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ical (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 three anthropogenic greenhouse gases, whereas the dataset supplied by CMIP5/PMIP3 uses data from multiple ice cores to derive the N₂O concentration. Both these differences in the experimental design are negligible.

We have added comments 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 CO₂, CH₄ and N₂O 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.

Page 9, line 7. Do you have the high frequency outputs to compute a time lag of about one month? Is it 30 days +/- a few days? Or between 0 to 2 months?

The model output was saved at monthly frequency. However, the time lag between the insolation anomalies and the SAT anomalies is between 0 and 2 months at almost all

latitudes. Exceptions occur over the sea ice zones in both hemispheres: in the Arctic Ocean, the response to the positive insolation anomaly which peaks in July is lagged by 3 months; in the Southern Ocean, there is no cooling in response to the negative insolation anomalies between December and July.

We have added comments to this effect.

Page 10, last paragraph. For the wavelet spectra, the significance of the spectra should be given.

We have revised Figure 6 accordingly.

Page 13. Here again, what the confidence/significance of the computed linear trends ? The paper mentions the signal-to-noise ratio problem, but nothing qualitative is shown.

We have revised Figures 9 and 10 so that only values significant at the 95% confidence level are shown. We have also made appropriate revisions to the text.

Page 13. Near the end : stong > strong

We have corrected this typographical error.

Page 16, top. Maybe you could put a word about ensemble simulations which may be a way to better asses the ENSO variability.

We have added a comment to this effect.

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Page 24 "However, as the model simulation does not account for the effects of anthropogenic aerosols, this may indicate that the transient climate sensitivity of Mk3L is too low" and page 28 "A low value for the TCR is consistent with the fact that the model correctly simulates the magnitude of the 20th century warming trend, despite not being forced with changes in anthropogenic aerosols (Section 4.5)". This is correct. However, I shall confess that I misinterpret this two sentences on my first two readings of the paper :-(A clue, and a reference, about the fact that aerosols reduce the model climate sensitivity could be useful for some readers.

We have revised both sections of text accordingly.

Page 28, bottom. You compare your "full AOGCM" to the slab oceans used before. Mk3L is flux corrected, and strongly corrected. I'm not convinced that it is less corrected than a coupled model with a slab ocean, which is a coupled model with "corrected" (i.e. fixed) ocean heat transport. This is not the aim of the paper to discuss that, but I would appreciate that here you remind the reader about this correction.

Despite the fact that Mk3L is flux-adjusted, there is a critical difference between the Mk3L AOGCM and a slab ocean model: In the Mk3L AOGCM, the thermohaline circulation — and therefore the ocean heat transport — is free to evolve; indeed, significant changes in the thermohaline circulation do occur within the simulations analysed in this section. In contrast, in a slab ocean model, the ocean heat transport is fixed.

Nonetheless, we agree that we should remind the reader that the model is flux-adjusted, and so we have added an appropriate comment.

Page 34. As the model is still drifting, you can not be certain that the shutdown is permanent, yet apparently more robust.

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We agree. We have revised the text accordingly.

Interactive comment on Geosci. Model Dev. Discuss., 4, 3363, 2011.

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