



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

Anonymous Referee #2

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Review of Phipps et al., “The CSIRO Mk3L climate system model version 1.0 – Part 2: Response to external forcings”

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.

For the 6ka BP simulation, reference to Cheddadi (1997) are outdated.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

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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

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.

Minor remarks

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

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?

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

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.

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Page 13. Near the end : stong → strong

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

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.

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 that 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.

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

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