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Geoscientific Model Development Discussions

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 authors describe the behavior of the CSIRO Mk3L climate model introduced in Part 1 under various "standard" forcing scenarios: mid-Holocene, transient Holocene, last millennium, and CO2-scenarios. The authors describe the model's response in terms of surface temperature, precipitation, and ENSO variability to orbital forcing applied in the time-slice mid-Holocene experiments and evaluate the influence of different acceleration factors of the applied orbital forcing in transient simulations of the last 6.000 yr. In the second part the authors investigate the models behavior to additional forcing, such as TSI, volcanic aerosols, and greenhouse gases in the last millennium. In the last past the model's response to different CO2-stabilization scenarios is investigated. The CSIRO Mk3L climate model behaves well within the range of paleo-reconstructions

and other comparable climate models under the respective forcing scenarios.

The paper is very well written and structured and describes the different experiments in a concise and straight forward manner. It succeeds in giving an overview of the model's performance under the different forcing scenarios and points out the potential shortcomings. It is, thus, a very suitable extension of the companion paper describing the model itself. From my point of view, there is one major concern the authors should discuss: How does the flux correction applied in the model affect the results obtained for potentially different mean states of the climate system in the mid-Holocene and also in the CO2-scenarios, especially in terms of the response of the ocean circulation discussed in chapter 5. If this point is addressed I would recommend the publication in GMD. Also, the authors might want to consider a few minor comments that I listed below.

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

In regard to the use of flux adjustments, we note that version 1.0 of Mk3L exhibits an excessively large rate of drift when used without them. As such, it is not feasible to conduct experiments that would allow us to directly determine the influence of flux adjustments on the response of the model. Nonetheless, we have revised the text wherever appropriate to cite studies that have analysed the influence of flux adjustments within other models, and to discuss the potential consequences of this for Mk3L.

We also note that version 1.1 of Mk3L, which has now been released, can be run without flux adjustments. We will evaluate this version of the model in a subsequent manuscript, which will compare the responses of the flux-adjusted and non-flux-adjusted configurations.

Minor comments:

For the mid-Holocene experiment, could you please state, why you chose a relatively short spin-up period of 100 yr? Since the control run itself has a non-negligible drift, how does the drift change, when you apply the forcing? And how large is the drift in the analysis period you consider?

As the differences in forcing between the pre-industrial control simulation and the mid-Holocene experiment are essentially seasonal in nature, 100 years was sufficient for the model to reach equilibrium with the changed boundary conditions. Between the 100-year spin-up period and the first 100 years of the mid-Holocene experiment, global-mean SAT decreased by just 0.006 K and global-mean SST decreased by less than 0.001 K. These rates of change lie well within the PMIP2 equilibrium criterion of 0.05 K/century.

During the 1000-year analysis period, global-mean SAT increases by 0.02 K in the mid-Holocene experiment and decreases by 0.08 K in the pre-industrial control simulation. These rates of change lie well within the PMIP2 equilibrium criterion and we consider them to be negligible.

We have added comments to the above effect.

For the different analyses performed, could you please indicate where the changes compared to the control run are significant? Also how dependent are the results to the analysis period you consider?

We have revised the figures, as appropriate, so that differences are only shown where they are significant at the 95% confidence level.

During the 1000-year analysis period considered for both the mid-Holocene and last millennium experiments, drift in the control simulation amounts to a reduction of only 0.08 K in global-mean SAT. This is now explicitly stated in the manuscript, and we consider this to be negligible. Analysis of the transient simulations of the late Holocene finds no evidence that the results are affected by drift, and this is discussed in the manuscript. While drift is a more significant issue in regard to the CO_2 stabilisation experiments, which span the full 4000 years of the control simulation, this is extensively discussed. In particular, the discussion of the equilibrium climate sensitivity presents values obtained both when drift is and is not taken into account (4.41 ± 0.02 K and 3.85 ± 0.02 K, respectively).

Why are ocean variables (temperature, sea ice, and deep-water formation) only discussed in the CO2-stabilization scenarios?

The manuscript focuses upon the most significant changes in each experiment. The changes in the ocean climate during the mid-Holocene, late Holocene and last millennium experiments are relatively small compared to the CO_2 stabilisation scenarios, and hence they do not receive extensive analysis.

Nonetheless, the revised manuscript now includes a section which analyses the changes in ocean temperatures during the late Holocene experiment. We have also added an analysis of the changes in the thermohaline circulation during the last millennium experiment. For all the experiments, the text now notes changes in SSTs, sea ice and the thermohaline formation where relevant.

In the difference plots, could you introduce a white interval around zero? Otherwise, it is difficult to decide whether a small signal is there, or not.

Where appropriate, we have revised the figures so that differences are now only shown where they are significant at the 95% confidence level.

Specific comments (Page and line numbers refer to the printer-friendly version of the manuscript.):

p.3367/l.5: Please give some representative citations.

We have added a representative selection of PMIP1 and PMIP2 publications.

3367/12: I think "Berger 1978" is the more appropriate citation here.

Braconnot et al. (2007a) is the most appropriate citation overall, as this documents all the external forcings which acted on the climate system during the mid-Holocene. However, given the dominant influence of orbital forcing, we have added Berger (1978) as well.

3367/14-17: Is the annual mean insolation dependent on the obliquity? I could imagine it is more the eccentricity of the Earth's orbit.

The referee is correct in stating that eccentricity does influence annual-mean insolation. However, the obliquity determines the meridional distribution.

We have revised the text to make this clearer.

3367/22-24: Not necessarily. The PMIP2 study (Braconnot et al. 2007a) suggests substantial changes in the annual mean as well.

Braconnot et al. (2007a) state "Changes are modest compared to those of the LGM, but reflect the sensitivity of the climate system to changes in the mean seasonal cycle of insolation. In particular, there is nearly no simulated change in annual mean temperature or precipitation for the Mid-Holocene, consistent with no change in global annual mean insolation.". However, we do agree that seasonal changes can be expressed as changes in the annual mean; in particular, the intensification of the African and Indian monsoons does lead to an increase in the annual-mean precipitation.

We have revised the text accordingly, and we have added a reference to Braconnot et al. (2007a).

3368/19-25: Maybe you can discuss the results obtained in Vamborg et al. (2011).

The effect of a dynamic background albedo scheme on Sahel/Sahara precipitation during the mid-Holocene, F.S.E.Vamborg, V. Brovkin, and M. Claussen Clim. Past, 7, 117-131, 2011

We have added this paper to the discussion.

3372/27-3373/5: If there is a westward shift, what happens if you choose Nino 4?

There is a 4% increase in the amplitude of the SST anomaly in the Niño 4 region. We have added this information to the manuscript.

3373/22+23: "boreal" instead of "northern"?

We have revised the text accordingly.

3376/14: How was significance tested here?

The original statement was based purely on visual inspection of Figure 8. Our intention was merely to state that any differences between the three simulations are consistent with the amplitude of internal variability. We did not intend to make any statements about statistical significance.

We have revised the text to make our meaning clearer.

3377/22-23 To my knowledge, the "abrupt desertification" of the Sahara in the Holocene is still under discussion (e.g. Kropelin et al. 2008, Brovkin and Claussen 2008)

Climate-Driven Ecosystem Succession in the Sahara: The Past 6000 Years, S. Kröpelin, D. Verschuren, A.-M. Lézine, H. Eggermont, C. Cocquyt, P. Francus, J.-P. Cazet, M. Fagot, B. Rumes, J. M. Russell, F. Darius, D. J. Conley, M. Schuster, H. von Suchodoletz, and D. R. Engstrom Science 9 May 2008: 320 (5877), 765-768. [DOI:10.1126/science.1154913]

Comment on "Climate-Driven Ecosystem Succession in the Sahara: The Past 6000 Years", Victor Brovkin and Martin Claussen Science 28 November 2008: 322 (5906), 1326. [DOI:10.1126/science.1163381]

We have incorporated both of these references into the discussion.

3378/26-28: I am skeptical that millennial-scale variability can be discussed using accelerated experiments.

We agree that this is dubious, and we have revised the text accordingly.

3379/2-4: If this is a robust feature, what could be the physical mechanism behind it?

The question of millennial-scale modulation of ENSO, and whether it is stochastic or externally driven, is beyond the scope of this manuscript. On balance, we have therefore decided to remove this sentence.

3383/15: Introduce abbreviation for equivalent CO2

We have revised the text accordingly.

3386/25: write "Earth"

We have revised the text accordingly.

3388/19-21: Can you show the evolution of the AMOC? How large is the heat transport associated with AMOC?

We have added a new figure (Figure 17 in the revised manuscript) which shows this. We have also added a discussion of the changes to the text.

3394/18-20: Please give the percent values as you do below.

We have revised the text accordingly.

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