

## ***Interactive comment on “The Southern Hemisphere semiannual oscillation and circulation variability during the Mid-Holocene” by D. Ackerley and J. A. Renwick***

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Reviewer 1 had mainly minor comments but raised some very useful points about providing better background to the SAO. These have been addressed in the following response (and will be included in the updated paper).

Page 186, lines 8 - 10: Rev comment: To put 'one model' in context it would be useful to state (at line 8) how many PMIP models (five or six) were involved in the analysis.

The sentence in the abstract will be changed to:

The output from the models suggest To

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The output from the five models analysed suggests

Page 187, paragraph 2, lines 7-18: Rev comment: Presumably the mid Holocene integrations were undertaken changing only the orbital parameters (e.g. GHG concentrations etc. were kept at the modern (preindustrial levels)). Make this explicit somewhere here. The interpretation of the induced changes depend on the latitudinal and seasonal changes in the incoming radiation. As such, the hemispheric average of these doesn't really convey much valuable information. I think it would be valuable (and help subsequent discussion) if the change (present minus M-H) was presented in a plot (graphs) showing the insolation changes through the year at a few key latitudes. This sort of material is presented in other papers (as cited), but is central to the argument here and the readers should have these in front of them. On a related point, it would be nice to see (perhaps in Section 1) some physical hypotheses formed as to how the SAO might be expected to change between these two epochs. There are vague comments made at various points, but a physical foundation would be useful before the results are presented and interpreted.

The authors agree with this suggestion and will include a new Figure (Figure 1 in this response), which shows the differences in SH insolation for the Mid-Holocene relative to the pre-industrial control. The following passage will be included from lines 14 - 17 (page 187) replacing the previous sentence starting- "The SH received" :

"The difference in insolation at the top-of-the atmosphere, averaged across all of the models used in this study, can be seen in Figure 1 for the SH. There is a strong decrease in insolation throughout the summer, which extends into autumn. The amount of insolation then begins to increase in late autumn (from the equator) before a rapid increase at high latitudes in the spring."

Also, the major change is the insolation, however, the levels of methane were changed from 760ppb to 650ppb (Mid-Holocene) but the major component is the insolation change.

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We have also included a more concrete hypothesis with the following changes after line 18, page 190, new paragraph: Based on the insolation changes given in Figure 1, we would expect to see a weaker CPT in summer to early winter with a strengthening in late winter into spring. However, as there is little overall change in the annual mean insolation (Braconnot et al., 2007), we would expect there to be little change in the interannual atmospheric variability in the Mid-Holocene compared to the present day.

Page 188, paragraph 1, line 3: Rev comment: The paper Simmonds and Walland (1998) in the references appears not to be cited in the text. In terms of what it conveys about the unforced variability of the SAO in a long coupled model run, this would be an ideal location to cite it.

The Simmonds and Walland (1998) paper will be cited at this point.

Page 188, paragraph 1, line 18: Rev comment: It is also worth citing in this context the paper by van Loon, H..J., J.W. Kidson and A. B. Mullan, 1993: Decadal variation of the annual cycle in the Australian dataset. *Journal of Climate*, 6, 1227-1231.

Citation read and to be included in line 21 with the following statement (just before Observational work sentence):

"Further work by van Loon et al (1993) confirmed that a change in the CPT variability occurred around 1979 and that this was not due to improved observations after 1979. Also, observational work"

Page 189, paragraph 1, line 10: Rev comment: There are additional factors which dictate that the calculation of time mean baroclinicity should be undertaken with caution. For example Lim et al (2009, Biases in the calculation of Southern Hemisphere mean baroclinic eddy growth rate. *GRL*, 36, L01707) have shown that down in the SH using time mean fields in the calculation (or estimation) of such baroclinicity (as is done here) is subject to significant bias. Their consideration of the temporal covariances (associated with synoptic passages etc.) results in greater growth rates and a southward shift

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in the latitude of greatest baroclinicity. This important perspective should be mentioned here, so the appropriate comparisons are made when interpreting the results.

Useful suggested reference of Simmonds and Lim (2009) and will be included. The passage now reads, from line 8:

"However, using seasonally averaged data may not be a good indicator of the baroclinicity of the atmosphere throughout that season as discussed in Simmonds and Lim (2009) and also reduced static stability in spring may lead to more cyclones (Walland and Simmonds, 1999)."

Page 189, paragraph starting at 1, line 13: Rev Comment: Over what domain are these EOF calculated? Were the modes rotated and, if so, why. While the use of EOF analysis here is reasonable, perhaps the authors could say a few words about the potential dangers in how these are interpreted. See e.g. Monahan, A.H., J.C. Fyfe, M.H.P. Ambaum, D.B. Stephenson and G.R. North (2009), Empirical Orthogonal Functions: The medium is the message, *J. Climate*, 22, 6501-6514. Dommenges, D. and M. Latif (2002), A cautionary note on the interpretation of EOFs, *J. Climate*, 15, 216-225.

The EOFs were calculated on area-weighted covariance matrices, and were not rotated. While rotation of EOFs can result in more "physically meaningful" and robust results (e.g. Cheng et al., 1995), they are used in their unrotated form here. This is largely because the SAM pattern tends to be most clearly expressed as the leading mode of unrotated EOF analysis. Rotation can isolate the zonally symmetric and eddy components of the SAM, but we feel this is not particularly physical in this situation.

The following will be added to the paper at line 15, page 189 as follows - "(EOF) analysis (see Thompson and Wallace, 2000, as an example). The EOF analysis was carried out using NCEP reanalysis data (from 1971-2000) defined on a latitude / longitude grid with spacing of 2.5 deg between 20S and 80S. Gridded data were area-weighted to account for convergence of the meridians. Unrotated EOFs were used, as the SAM

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pattern is most clearly identified in unrotated EOF analysis, and for consistency with other studies (e.g. Thompson and Wallace 2000, Kidston et al 2009). The three leading modes of SH interannual variability"

Refs for info: Cheng, X., G. Nitsche, and J. M. Wallace, 1995: Robustness of low-frequency circulation patterns derived from EOF and rotated EOF analyses. *J. Climate*, 8, 1709-1713. Kidston, J., J. A. Renwick, and J. McGregor, 2009: Hemispheric-scale seasonality of the Southern Annular Mode and impacts on the climate of New Zealand. *J. Climate*, 22, 4759-4770.

Page 190, paragraph 1, line 22: Rev comment: 'SH' is already defined (p187, 1. 12).

corrected to just SH as suggested.

Page 191, paragraph 2, lines 4-5: Rev comment: Delete this sentence. NH folks are OK with the reference to austral seasons!

Changed as suggested by reviewer.

Page 196, paragraph 1, line 6: Rev comment: Change 'The changes is' to 'The structure of the changes in'

"The changes is" will be changed to "The structure of the changes in.."

Page 197, paragraph 2, lines 1-4: Rev comment: In this discussion of the influence of (changes is) SAO on New Zealand it would be worth reminding the reader that the amplitude of the SAO in that region is rather small (see Simmonds 2003, Modes of atmospheric variability over the Southern Ocean, *JGR*, 108, 8078, doi: 10.1029/2000JC000542) in the NCEP reanalysis. Also, I have trouble with the manner in which the last sentence is expressed. It appears to say that MAM and SON are directly connected to the SAO, whereas it is wavenumber 2 of the annual cycle. Reword more appropriately.

Lines 3 and 4 will be deleted and the following passage included: "The focus will be on

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MAM and SON where there appears to be the largest changes in the wavenumber 2 of the annual SLP cycle. However, the amplitude of the SAO in the NZ region is relatively small compared to the rest of the SH mid-latitudes (see Simmonds, 2003) and so other factors may influence the climate in this region more."

P197, paragraph 2, lines 27-28: Rev Comment: Cite papers in chronological order. (also at p. 202, 1. 4.).

Papers re-arranged in to chronological order and also p202, para 1, line 4.

P199, Section 4.2: Rev comment: Do we really need this subsection of the annual mean? Does it add to the purpose of the paper?

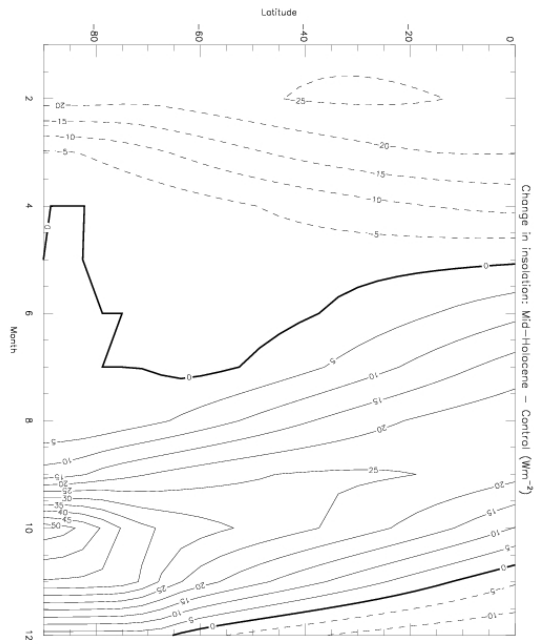
The authors agree with the reviewer and as we would like to include some more analysis and figures in response to reviewer 2's comments (plus the insolation figure here) we will remove the annual mean section (4.2) and figure (12).

Figure 1: The difference in zonal mean seasonal insolation for the Mid-Holocene relative to the control, averaged over all seasons in the run ( $Wm^{-2}$ ). Positive (negative) values are indicated by the solid (dashed) lines. The zero line is given as the thick black line.

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**Fig. 1.** Figure A: The difference in zonal mean seasonal insolation for the Mid-Holocene relative to the control, averaged over all seasons in the run ( $Wm^{-2}$ ).