

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

Anonymous Referee #1

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Review of ‘The Southern Hemisphere semiannual oscillation and circulation variability during the Mid-Holocene’ by Ackerley and Renwick

This is a valuable paper exploring one of the key modes of atmospheric variability (namely the semiannual oscillation) in the Southern Hemisphere, and how well its character is modeled in coupled models run for present ‘preindustrial’ climate and what changes it exhibits in the mid-Holocene. It provides sufficient new material and insights to warrant publication in *Climate of the Past*. There are a few aspects of the paper which should be revised to produce a very valuable document:

Page 186, line 10: To put ‘one model’ in context it would be useful to state (at line 8)

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how many of the PMIP models (five or six?) were involved in the analysis.

p. 187, paragraph II. 7-18: 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.

p. 188, I. 3: 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 the ideal location to cite it.

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

p. 189, I. 10: 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. Geophys Research Lett*, 36, L01707) have shown that down in the SH using time mean fields in the calculation (or estimation) of such mean baroclinicity (as is done here) is subject to significant bias. Their consideration of the temporal covariances (associated

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with synoptic passages etc.) results in greater growth rates and a southward shift in the latitude of greatest baroclinicity. This important perspective should be mentioned here, so the appropriate comparisons are made when interpreting the results.

p. 189, paragraph starting at l. 13: Over what domain were 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.

p. 190, l. 22: 'SH' already defined (p.187, l. 12).

p. 192, ll. 4, 5: Delete this sentence. NH folks are OK with the reference to austral seasons!

p. 196, l. 6: Change 'The changes is' to 'The structure of the changes in'.

p. 197, ll. 1-4: In this discussion of the influence of (changes in) the 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.

p. 197, ll. 27,28: Cite papers in chronological order. (Also at p. 202, l. 4.)

p. 199, Section 4.2: Do we really need this subsection of the annual mean.? Does it add to the purpose of paper?

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