Geosci. Model Dev. Discuss., 4, C843–C846, 2011
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Geoscientific Model Development Discussions

Interactive comment on "Simulating Southern Hemisphere extra-tropical climate variability with an idealized coupled atmosphere-ocean model" *by* H. Kurzke et al.

Anonymous Referee #1

Received and published: 7 October 2011

Review of the manuscript entitled "Simulating Southern Hemisphere extra-tropical climate variability with an idealized coupled atmosphere-ocean model" by Kurzke, H., M.V. Kurgansky, K. Dethloff, D. Handorf, D. Olbers, C. Eden and M. Sempf

This paper introduces a new idealized coupled atmosphere-ocean model based on a simplified quasi-geostrophic atmospheric model coupled to a global dynamical model of the ocean with simplified physics. The atmospheric model is a hemispheric model and the coupled system has been applied for Southern Hemisphere conditions of the Earth. The model is run over 1000 years in order to analyse aspects of its climatology and low-frequency climate variability.

The presented work demonstrates an important contribution to the development of simplified dynamical climate systems that allow cost-effective simulations of the coupled atmosphere-ocean dynamics on long time scales. It brings together research efforts that have been made by the authors over a number of years in terms of idealized atmospheric and oceanic circulation models and thus provides the opportunity for including more fundamental realism in assessing the internal modes of variability either in both sub-systems or in the coupled ocean-atmosphere system.

The paper is well written and structured.

My main criticism relates to the analysis of the model variability and the physical processes involved. The section on Model Variability was rather disappointing. It is clearly the weakest part of the paper and needs a major re-write, see comments below. I would recommend publication after the comments will have been addressed adequately.

Major Comments:

The motivation for constraining the model to the Southern Hemisphere only is not very clear and needs more discussion. Why has the model not been applied to the Northern Hemisphere?

The model performance in terms of simulating observed modes of extra-tropical variability is over-stated. For example, the leading EOFs of the uncoupled and coupled simulations do not bear much resemblance to the observed ones (Fig 6). Also, their explained variances seem significantly smaller than in the re-analysis. Have the authors checked whether EOF1 can be distinguished, in a statistical sense, from the following EOFs and whether these show any more similarity to the observed pattern? The overstatement also applies to the conclusions on page 15, line 3.

Based on Fig 5, a possible climatic trend is mentioned but it remains unclear whether this is just a spurious artefact or whether there is a real physical process behind it. The discussion about the model variability related to Fig 5 is neither well structured nor well

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argued. E.g., on page 13, line 17, it is stated that the priority of the analysis is put on BARBI which seems out of context and misleading given the coupled processes in the model and that you discuss the atmospheric component of the variability in some of the figures. Further, Fig 7 and 8 are only mentioned by passing leaving the reader with no understanding of what you were supposed to show here. Fig 9 is not even mentioned in the text. Fig 10 is poorly explained and discussed. Why and how does it show that the slow BARBI subsystem effectively integrates the fast forcing? Please give more evidence for this. The discussion on page 14 unfortunately does not help in shedding more light on this question. Perhaps it would be helpful to put your results in the context of more observational evidence and finding in more complex and realistic coupled AO-GCMs.

I felt that the rather broad statements about the effect of the reddening in the spectra lack some specific discussion, e.g. with respect to figure 7.

Minor comments:

The authors describe a reduction of approx. 20% in the Drake Passage transport in the coupled runs compared to the forced ocean-only runs. What is the reason for this?

Page 4, line 17: add “re-analysis” after NCEP-NCAR

Page 5, line 10: specify which re-analysis has been used (ERA-40? ERA-Interim?)

Page 10, line 2: explain the reduction coefficient

Page 10, line 8: There is no such thing as the “standard wind speed at 10m height” – please explain

Page 11, line 10-15: This sentence is not well structured.

Fig 3: The lines from subplots b-d) could be combined in just one plot with 3 curves for the three locations.

Fig 4: change “(dashed)” to “(dotted)”

Fig 11a) How does this spectrum differ from that shown in Fig 7b) except for the different time units? What domain is it computed for?

Fig 11: The fitted red noise curves in the 4 subplots are very different and I would appreciate some comment or discussion on this.

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