Geosci. Model Dev. Discuss., 4, C1136–C1139, 2011
www.geosci-model-dev-discuss.net/4/C1136/2011/

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 #2

Received and published: 1 December 2011

Review of the paper entitled "Simulating Southern Hemisphere extra-tropical climate variability with an idealized coupled atmosphere-ocean model" by Kurzke et al.

The authors present a new coupled model resulting from the coupling of the BARBI ocean model to a three-level quasi-geostrophic model. Because BARBI does not simulate explicitly temperature and salinity, some specific treatments at the atmosphere-ocean interface are needed. Those points are well described in the paper. The present version of the paper, however, fails in explaining to the reader the main reasons of this new model development and of the choice of its atmospheric and oceanic components. Those choices must be justified in detail before any final publication to show why and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

how this new model should be used.

Major points

1/ After reading the manuscript, it does not appear clearly to me why this new model has been developed. This was apparently a hard task (as mentioned page 1923, line 1). The model has still clear biases, as illustrated for instance in Fig. 6 by the comparison of the first EOF of geopotential height at 833hpa of the model with the observed one, that can pose problems in the interpretation of the results. The goal is to study the role of atmosphere-ocean interactions in internal climate variability. In this case, why not using an existing model? What is the advantage of this new one? It is possible to perform long simulations with this model (1000 yr), but it is also the case with many other ones now. The simplifications applied in the dynamics may help to understand the dominant processes but the authors should explain the reader how. I admit that the goal of the paper is not to analyse the internal variability in the Southern Ocean but to present the new model. The proposed analysis is however much too superficial at this stage. It does not allow the reader to see the strength and limitations of the approach. The revised version should then give a deeper analysis demonstrating by this example that the new model brings information hard or impossible to obtain with another model.

2/ I do not understand how the atmospheric model was "adapted to the Southern Ocean". From some parts of the paper, I guessed that the model was regional with the ocean model driven by reanalysis outside the atmospheric model domain (e.g., page 1911). However, page 1912, line 20, it is said that the model has 32 points in the meridional direction, with 16 north of the Equator. Does it mean then that the model is global or are those numbers valid for the original version before its adaptation to the SH (page 1913, line 8)? This must be clarified.

3/ The manuscript includes many imprecise sentences and small errors that made the reading difficult for me. Some specific examples are given below but many sections of the paper would benefit from some rewriting.

GMDD

4, C1136–C1139, 2011

Minor points

Page 1910, lines 3-5. It should be mentioned that the ACW was not observed during the last decade.

Page 1910, line 11-18. This sentence is very long and hard to follow. Please rephrase.

Page 1910, line 19. Please define at this stage the baroclinic potential energy.

Page 191, line 27. I do not understand the meaning of "this interaction occurs indirectly". In the model, interhemispheric connections are only possible through the oceanic circulation. This is one of the potential connections, not an indirect one.

Page 1915, line 21. Why is it said that the formula is linearized while Equation (1) is not linear?

Page 1918, line 14. It is said that Figure 5 is for the last 10yr of the experiment but I see 1000 year time series on panels b-d.

Page 1918, line 17. What is meant by 'Figure 5b-d demonstrate temporal behaviour of the streamfunction" ?

Page 1918, line 20. Why discussing figure 9 before 6-8 ?

Page 1918, line 19. From Fig. 5, PSI is not increasing southward in a monotonic way (there is a gyre in the Weddell Sea for instance).

Page 1918, line 25. Is the number given (20 Sv) related to the variability of PSI or to the transport through the passage. If it is the variability of PSI, an interpretation of this value should be proposed as this is not straightforward to me. A clearer link with the discussion of the transport given in the first paragraph of page 1919 should also be included.

Page 1919, line 25. There is a reference to Fig. 8 but I do not see any link with the text. If shown, fig. 8 should be discussed.

GMDD

4, C1136–C1139, 2011

Interactive Comment

Page 1920, line 3. The mentioned feedback should be quantified (or at least shown) by comparing fig. 7 and fig. 11 (if I understand well fig. 7, see below, otherwise use another figure). From the information given, the reader can just trust the authors on the basis of this very general sentence.

Page 1920, line 23. The wavelet spectra is not discussed. Fig. 12 is thus useless and should be suppressed.

Fig. 5. The digits 1,2,3 are not on panel a. I guess the reference should be to Fig. 3a

Fig.7. What is the variable analysed?

Interactive comment on Geosci. Model Dev. Discuss., 4, 1907, 2011.