<u>Response to comments of reviewer 2</u>

We thank the reviewer for his careful review and the suggestions. We implemented the changes in the revised manuscript of 8 th December 2011 and these are indicated in bold.

Major Comments:

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). 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**.**

On page 5 we added new sentences in our manuscript:

The main aim of this work is the development of a simplified dynamical climate system coupling atmosphere and ocean subsystems including the essential nonlinear dynamical features and increased realism with respect to internal coupled climate modes. This novel model bridges previous research dealing with the construction of idealized atmospheric and oceanic circulation models.

We added the new Figure 6 d-f, which displays the 2. EOF for the atmosphere-only run, the coupled atmosphere-ocean simulation and from the NCEP data as in Fig. 6a-c. The overall agreement in the spatial pattern between the coupled model and the observations is much better than in the 1. EOF but the explained variance is slightly weaker in the coupled run (5%) as in the NCEP data (8.4). The total variance explained by the two model EOFs is now closer to the total observational variance of EOF 1, 2.

Figure 6. (d) Second EOF of 833 hPa geopotential height for the 1,000 year run of the atmosphere-only model (5.9% of explained variance). (b) Second EOF of 833 hPa geopotential height for the 1,000 year run of the coupled model (5.0% of explained variance). (c) Second EOF of observed austral wintertime geopotential height in the lowest model level (NCEP-NCAR reanalysis data for 1948-2003) (8.4% of explained variance).

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.

On page 5, we added the following sentences:

A hemispheric atmospheric model is considered, assuming anti-symmetric streamfunction with respect to the equator. Thus the model is considered for the whole globe, but only its SH part is used to interact with the Southern Ocean. In the NH no interactive coupling between the atmospheric and the ocean model occurs and NCEP winds force the oceanic circulation there.

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.

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

Done.

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

Page 1910, line 19. Please define at this stage the baroclinic potential energy.
Reference to Appendix A was introduced.

Page 1911, 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.
Done.

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

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.
Fig. 5a shows the last 10 yr, but 5b-d present the 1000 year long runs. The figure caption was rephrased.

Page 1918, line 17. What is meant by ’Figure 5b-d demonstrate temporal behaviour of the streamfunction" ?
This sentence was rewritten on page 12.

Page 1918, line 20. Why discussing figure 9 before 6-8 ?
Done on page 14.

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).
Done.

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.
Rewritten on page 12.

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.

Done on page 14. Fig. 8 describes the spatial pattern of the zonal and meridional components of the wind stress climatology in the coupled and the ocean-only model simulations over 1000 years.

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.
We removed the mentioned phrase and improved the text in the following way.
"At long time-scales, the ocean is also directly forced by the wind stress, because the spectra of atmospheric fluctuations extend to very low frequencies. Redistribution (propagation) of variability towards long time-scales in the coupled model is clearly seen in Fig. 7b if compared with Fig. 7a for the atmosphere-only model. Thus enhanced ultra low-frequency variability evident in Fig. 7b leads, in turn, to an increased wind forcing that drives BARBI on very long time-scales. On those time-scales BARBI possesses its own intrinsic non-linear dynamics, due to the non-linearity inherent in equation (A2). So, an intricate positive feedback loop develops, which provides unique characters to low-frequency variability within this coupled model."

Page 1920, line 23. The wavelet spectra is not discussed. Fig. 12 is thus useless and should be suppressed.
We describe now Fig. 12 with the following sentence.

Fig. 12a displays the wavelet spectrum for the atmospheric PC 1 of geopotential height at 833 hPa over 1000 years coupled simulations. Figs 12b-c show the wavelet spectra for the 1st and 2nd PC of SST' over the ACC. Fig. 12b indicates a statistically significant multi-decadal peak around 100 year periods in the ocean SST and Fig. 12c indicates similar variations on decadal time scales

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

Numbers 1-3 are introduced in Fig 5.

Fig.7. What is the variable analysed?

833 hPa geopotential heigt was analysed and figure capition changed.