<u>Response to comments of reviewer 1</u>

We thank the reviewer for his careful review and the offered suggestions. We refer to the Discussion Paper in GMD and discuss all changes with respect to that manuscript version. Text in red are changes implemented into the manuscript.

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?

On page 1911 after LN 23, we added a sentence in our manuscript:

The principal aim of this study is to investigate intrinsic extra-tropical sources of climate variability in the Southern Hemisphere. Constraining the atmospheric model to the Southern Hemisphere only is necessary owing to quasi-geostrophic model approximations. The Southern hemisphere is chosen because the oceanic BARBI component of the coupled model most successfully simulates the wind-driven oceanic circulation in the SH.

On page 1921 after LN 6, we added the following sentences:

The oceanic BARBI model successfully simulated the wind-driven oceanic circulation in the SH, but in the used simplified set-up it has certain weaknesses in reproducing, e.g., the intensity of oceanic gyres in the Northern Atlantic, where the thermohaline processes are of primary importance. Therefore, we are in a situation when the used simplified version of BARBI model simulates well the SH oceanic circulation, whereas the atmospheric model with more ease would reproduce the key features of low-frequency atmospheric variability over the NH, where the ocean-continent contrasts are by far more pronounced. Even in this situation the described coupled model has revealed a realistic performance. Constraining the model to the Northern Hemisphere only would need precautions, because of the aforementioned difficulties of the used simplified version of BARBI model to simulate the NH oceanic circulation, and the appropriateness of restricting oneself to a simplified BARBI version in order to be able to couple it in a feasible way to a quasi-geostrophic atmospheric model."

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?

We introduced the following sentences on page 1919, LN: 23

It was tested whether the first EOFs are well separated from each other by applying the "Rule-of-Thumb" of North et al. (1982). The sampling errors of the respective eigenvalues for the first four EOFs are smaller than the distance between the neighbouring eigenvalues for the amosphere-only run, the coupled runs as well as for the NCEP-data

On page 1919, LN 15 the sentence describing the AAO pattern was changed and new sentences have been introduced. We show now also the 2. EOF for the atmosphere-only, the coupled atmosphere-ocean simulations and the NCEP data.

In the coupled model simulations, the AAO pattern is only partly reproduced versus observations at 833 hPa atmospheric model level (Fig. 6b-c); the corresponding first EOF for a 100yr GS3LM-only run is shown in Fig. 6a. The smaller explained variance in the first and the second EOF of the atmosphere-only and in the coupled model setup can be contributed to the coarse horizontal resolution of the oceanic model and of the atmospheric model. As stated by Fanning and Weaver (1998) a higher ocean resolution is important for the excitation of stronger decadal-scale variability in idealized coupled atmosphere-ocean models due to the reduced parameterized diffusion. Goose et al (2010) mention a too weak atmospheric circulation in an Earth system model of intermediate complexity which contains a spectral three-layer atmospheric model similar to our approach but with a different ocean model.

Fig. 6d-f 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 is 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).

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).

The overstatement also applies to the conclusions on page 15, line 3.

On page 1921, LN 3 and following was changed to:

By coupling the hemispheric …, it was possible to reproduce some large-scale features of the extratropical atmospheric processes in the Southern Hemisphere, although the explained atmospheric model variance is too small compared with observations. The model variance could be increased due to higher ocean model resolution with a reduced parameterized diffusion as stated by Fanning and Weaver (1998). The transport properties of the Southern Ocean circulation are in qualitative accord with the essential features of observations.

Based on Fig 5, a possible climatic trend is mentioned but it remains unclear whether this is just a spurious artifact or whether there is a real physical process behind it.

There are intrinsic minor imbalances in the model, related to the differences in the mode of driving the oceanic circulation over the SH and NH, respectively. This might result in a gradual re-distribution of energy between the hemispheric oceans and spinning-up of the SH oceanic circulation. This is indicated in the manuscript, on lines 3-5, page 1919. Because the exact reason for the imbalance in question remains unknown, we restricted ourselves with that general statement.

On page 1919, LN 5 we introduced the following sentences:

The understanding of the physical processes is limited due to the coarse applied resolution in the atmosphere and in the ocean and would need experiments with higher resolution coupled GCMs.

The discussion about the model variability related to Fig 5 is neither well structured nor well 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.

Page 1919, starting from LN 17-23 was changed to.

The oceanic BARBI model has been proven efficient in simulating the predominantly wind-driven Southern Ocean circulation, and the lower-level-only information from GS3LM is used to compute the acting wind stress. Therefore, the AAO as the leading mode of atmosphere-ocean dynamic interaction on long time scales and at mid- and high SH latitudes is reproduced satisfactorily, which gives an incentive to use the constructed coupled model in this study.

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.

Sorry, but Figure 9 was described in the text on page 1918 (LN 20-24):

"Figure 9a presents 1000 yr mean climatology of the baroclinic potential energy *E*, whereas the field of SST-deviations from the basin average value (SST′) is shown in Fig. 9b. There is a high correlation between *E* and SST′ but the impact of variable oceanic depth *h*, see Eq. (B1), is clearly visible, too."

On pages 1919 LN 24 and following we changed the text slightly and added a sentence with a citation:

In our experiment, BARBI is forced by a chaotically behaving GS3LM with most effect on synoptic time-scale (Fig. 7). On this time-scale, the fluctuating wind stress (its climatology is presented in Fig. 8) drives the fast barotropic BARBI-subsystem, where the transport streamfunction Ψ serves as the fast variable (Fig. 5b–c). The slow baroclinic BARBI-subsystem, quantified by the baroclinic potential energy *E* and JEBAR-term, effectively integrates the fast "stochastic" forcing implemented by the wind stress and the barotropic pumping action of the mean stratification (the first term on right-hand-side of (A2)). On these long time scales the ocean imposes a feedback on the atmospheric circulation, through SST-variations which are inferred from the slow *E*-variable that dominates the low-frequency variability in BARBI. The separation of slow and fast responses in BARBI has been demonstrated in Olbers and Lettmann (2007).

The discussion of the results of more complex AOGCMs with respect to explained variance patterns in e. g. IPCC AR4 AR simulations is beyond the scope of this paper.

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.

A more specific discussion with respect to Fig. 7 was presented on page 1921, LN 7 and following:

"Due to the coupling, there is significant oceanic low-frequency variability apparent in the SST anomaly (SST′) time-variations and demonstrated in Figs. 10–12. The first EOF of the simulated SST′ shows a similarity with observed patterns (e.g., Doney et al., 2003, 2007). The spectral analysis of the temporal evolution of the dominant variability patterns makes it clear that while the power spectra for the atmosphere-only model are more white (Fig. 7a), the power and wavelet spectra of atmospheric variables for the coupled model demonstrate more apparent red behavior (Figs. 7b, 11a and 12a).

On page 1920, LN 7-9 we introduced the new sentences:

"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."

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?

We explained that now with the added sentence on page 1918, LN 17:

"The reason for this is primarily due to a northward shift of the maximum of westerlies over the south-east Pacific in the atmospheric model compared to observation; compare Figs. 8a and 8c."

Page 10, line 2: explain the reduction coefficient

We introduced on Page 1916, LN 2:

According to the thermal wind equation written in isobaric coordinates, the horizontal wind velocity depends logarithmically on the pressure if the horizontal temperature gradient is approximately height-independent. Based on this assumption, the reduction coefficient value, $k = 0.7$, was determined by a semi-empirical choice. The reduction coefficient $k = 0.7$ means that, on average, the 10m wind speed constitutes 70% of the wind speed at the lower model level of 833 hPa.

On page 1925, LN 26 we introduced the sentence:

We thank an anonymous reviewer for his careful review of the manuscript.

On page 1926, LN 21 we introduced the new citation:

Fanning, A. F., and Weaver, A. J.: Thermohaline variability: The effects of horizontal resolution and diffusion, J. Climate, 11, 709, 715, 1998.

On page 1926, LN 26 we introduced the new citation:

Goose, H. et al.: Description of the Earth system model of intermediate complexity LOVECLIM version 1.2, Geosci. Model Dev., 3, 603-633, 2010.

On page 1927, LN 22 we introduced the new citation:

North, G. R., Bell, T. L., Cahalan, R. F., Moeng, F. J.: Sampling errors in the estimation of empirical orthogonal functions, Mon. Weather Rev., 110, 699-706, 1982.