

Interactive comment on “Simulated pre-industrial climate in Bergen Climate Model (version 2): model description and large-scale circulation features” by O. H. Otterå et al.

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We thank the reviewer for the useful comments. The main change made to the manuscript in response to the reviewers' comments include a more detailed discussion on the warm-bias in the Southern Ocean, as well as the circulation in the SO with respect to other climate models (Russel et al. 2006). Also, the possible impact of the SO error on the model ocean heat and tracer uptake in future climate change simulations are now briefly discussed in the text. In addition several minor changes has been made and some of the figures have been modified as requested. We believe that we have responded satisfactorily to the reviewer's comments, which has contributed to an

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improved manuscript.

Detailed answers to the reviewers' comments follow:

Major comments:

1. My only general suggestion is that the authors include more discussion of the impact of the Southern Ocean on the model oceanic heat and other tracer uptake in future climate change integrations.

A detailed discussion of the impact of the SO error on future climate change simulations is beyond the scope of this paper. The reviewers comments on potentially less oceanic heat and tracer uptake in future climate change simulations have, however, been included in the Discussion section. For a more detailed discussion on the regional carbon-climate feedbacks in BCM we refer to the paper “Assessment of regional climate-carbon cycle feedbacks using the Bergen earth system model (BCM-C)” by Tjiputra et al. 2009 currently in open discussion at GMDD.

2. It would be helpful if the authors perform an analysis similar to that found in Russell et al. 2006. How bad is the SO simulation relative to other models?

Following Russell et al. 2006, the distribution and magnitude of the simulated wind stress over the Southern Ocean have been included together with observed values in a new figure (see revised manuscript). The peak zonally averaged wind stress is shifted equatorward relative to the observations (from NCEP long-term mean), and is about 30% weaker than observed. The study by Russell et al. 2006 shows that many of the IPCC coupled climate models suffer from similar problems as BCM. In particular, all but a few of the models show an equatorward shift of the peak westerlies in the Southern Ocean, as is the case in BCM. The text has been updated accordingly.

Specific comments:

Section 2.3, sea ice model: More details are needed here. What grid is the sea ice model on? What scheme is used to advect the ice? Is there a ridging scheme?

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The following text has been added to the sea-ice model description: “For every thickness category the model computes sea-ice velocities solving the stress tensor as described in Hunke and Dukowicz (1997) and then applies a non-diffusive advection scheme that conserves the first and second moments of the advected fields (Prather, 1986). Redistribution of sea-ice due to rafting and ridging processes is simulated according to the theory developed by Thorndike et al. (1975). In order to model vertical heat diffusion in an ice-snow slab, a vertical discretization is defined, considering four layers in the ice part of the slab and one level snow. Solving of this diffusion scheme is implicit in time. All model equations are solved on an Arakawa B-grid which shares its grid points with the C-grid of the ocean model. A linear interpolation between the two grids is performed internally by the sea-ice model. The rest of the thermodynamics, including snow aging scheme is treated explicitly (Salas-Melia, 2002).”

P515, line 2: According to Stouffer et al 2004, 150 years is too short for the spin-up. Comment? Although dictated by available CPU time at the time, we agree that 150 years is too short and should probably have been extended by a few more centuries. The model drift is largest during the first 200 years, while it is quite stable for the last 400 years (Fig. 1). For future studies it would therefore be a good idea to extend the control run with a few centuries to get a more stable control run. Sensitivity studies based on this control run should also ideally start a few centuries into the control run. It should be noted, however, that the overall model drift is much smaller than in previous versions of the model, and thus constitutes a clear improvement.

P516, line 19: How is the mass flux correction added into the model atmosphere?

The following method has been used for the mass flux “correction” in the model (text added to manuscript): “In ARPEGE there is a mass drift due to the non-conservative form of the discretized continuity equation (M. Deque, pers comm.). In order to correct for this the following method has been used: every 6 hours the partial pressure of dry air averaged over the globe is calculated. This value is then compared with the value 983.2 hPa, which is the ERA40 average value. The difference (loss or gain of

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mass) is then added to the surface pressure uniformly (less than 0.1 hPa per month). With this correction total air mass can have a seasonal cycle or a long-term drift (due to variations in water vapor content), but no drift due to non-conservation due to the equation. The drawback, however, is the uniform correction, since the sources and sinks may be local.”

P517, lines 15-23: Does the water budget close in the model?

The following text has been added: “In the model the water cycle budget is not entirely closed. There is therefore a drift towards higher global-mean salinity during the whole integration. The overall drift is about 0.01 psu per century for the whole 600 year integration. Over 80% of this drift is balanced by growth of the Greenland and Antarctic ice sheets due to the absence of a calving scheme in the model. The remainder is likely due to the non-conservation of mass in the atmospheric model.”

P517, line 25-29: Mark observed values on figure 4.

The observed values have been added to the figure.

P518, line 1-15: What is the authors’ assessment of the Southern Ocean problems?

The discussion of the SO error have been totally rewritten (see response to Reviewer #1). Basically, we now believe excessive mixing between the surface and the deep ocean in the Southern Ocean is the main cause for the warm bias and underestimation of sea ice extent in this region. The excessive mixing erodes the halocline and makes it difficult to maintain a fresh and cold surface layer required for wintertime freezing of sea-ice. The underestimation of sea-ice will in turn cause a warm bias in the model due to increased absorption of short-wave radiation.

P518, line 1-15: The phrase “albedo of low clouds in sea ice model” makes no sense.

This part has now been removed.

P519, line 8: Add “realistic” after “shallow”.

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Done.

P519, line 10: Any peaks in the spectrum of the AMOC?

The Atlantic MOC and its variability is the subject of several ongoing studies. Nevertheless, we have added the power spectrum of AMOC to Figure 6.

P520, lines 20-25: Add the global RMS value for the SST error tot the discussion.

The global RMSE values for SST and SSS have been added to figure 7.

P521, SLP figure: Show the model minus observations differences as a third panel.

A third panel showing the suggested anomalies have been added to the SLP figure.

P522, line 12: Is there a reference for the gravity drag parameterization?

References have been added to the text.

P523, precipitation figure: Why cut the figure at 60N?

Figure have been modified to include the entire Earth.

P524, ENSO discussion: This discussion is not clear.

The reviewer makes some good points here. In light of this, the discussion of ENSO has been rewritten to include the reviewers' comments. As requested by Referee #1 some simple statistics such as skewness and kurtosis have also been added to the discussion. In order to address the reviewers' comment on possible impacts on teleconnection patterns, the following text has been added to the text: "The simulated pattern shows a similar structure as in the observations, but with much weaker amplitude (Fig. 13d). The combination of a too weak ENSO and the cold tongue error along the equator in the Pacific (Fig. 7a), means that the precipitation does not move much during El Nino events (not shown). This could potentially have an impact on teleconnection patterns. A more detailed analysis of the tropical variability in the model will be reserved for future studies."

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P527: Add discussion of the impact of the Southern Ocean simulation errors on ocean heat and other tracer uptake in climate change simulations using this model.

See major comment.

P528, line 11: No. XXXX. Fix the number.

Number will be added upon acceptance of the manuscript for publication.

Stouffer et al. 2004 is missing from the reference list.

The reference has been added.

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