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Geoscientific
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Discussions

Interactive comment on "An Intermediate Complexity Climate Model (ICCM) based on the GFDL Flexible Modelling System" *by* R. Farneti and G. K. Vallis

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We thank the reviewer for his/her comments on the manuscript, detailed review and careful reading. The following is a list of the reviewer's minor comments and technical corrections, and our answers to his queries.

1) General Comments:

- We do not agree with the Referee's main concern on the brief inclusion of different experiments and configurations, although we appreciate his interest. The Referee points out that, on page 351, we suggest that more analysis on the different experiments will be presented elsewhere, and questions the opportunity to 'trailer' some preliminary re-

sults here. The sentence on page 351 is indeed misleading. What we meant is that we will present original scientific results based on the different Atlantic experiments in following publications, and we will thus only show the climatology and significant differences among experiments in this study. In fact, so far, we have submitted two papers on the decadal variability and partitioning of energy transport in the system. Those results do not pertain to the Journal 'Geoscientific Model Development' and we envisage that many more studies will spawn from these and similar experiments. Moreover, we strongly believe on the importance of presenting some climatology of Aquaworld simulations because we want to i) make clear that the model is not restricted to the particular (sector) geometry of the Atlantic case [see Referee's comment about page 342-line 9] and ii) show other possible interesting configurations on the global and not basin domain.

We have modified the sentence in page 351-line 6 to the following: *"Here we mainly restrict our attention to the Control (CTL) solutions, as the main focus of the present paper is on the description of a base integration. Furthermore, we tested the climatic response to variations of some fundamental parameters of the climate system such as the moisture content in the atmosphere, the meridional gradient of the incident solar flux and the Earth's rotation rate. Various numerical experiments and analyses on the climate variability, energy transport in the coupled system and their dependence on oceanic, atmospheric and planetary parameters and dimensions will be presented elsewhere."*

- The ICCM source code is freely available and can be obtained with the ocean model MOM. We added the following at the end of Section 2.1: *'The model is now part of the GFDL FMS repository and will be thus supported and developed for future releases; ICCM can be freely obtained with each standard release of the ocean model MOM.'*

- We now refer to the model as ICCMp1 to clearly identify its version.

2) Specific Comments:

- 342, line 9: The idealised geometry and geography are indeed a contributing factor in the computational savings, both for the Atlantic and Aquaworld experiments. They are by no means 'hardcoded' in the model and, as stated in the text, one can set up any geometrical configuration, from sector to global, with different flavours of land representation. As suggested by the Referee, we removed from the Abstract the comment on idealised geometry and geography, as it might lead the reader to think that they are a fundamental property of the model.

- 342, line 12: we have consistently changed these definitions. The length of the runs are on the order of 'multiple millennial' while the time scale of study is 'interannual to centennial'.

- 343, line 2: We have stated more clearly that our focus is on coupled ocean-atmosphere models for the study of climate variability. We now say: *'The most realistic tools for the study of climate variability and change are state-of-the-art, 'IPCC-class' coupled general circulation models'*.

- 343, line 12-18: Thank you for the interesting reference. However, we believe the main 'simplification' of their model is in the coupling, which is a 'periodically synchronous coupling', allowing for considerable saving in computer resources.

- 343, line 22-29: Thank you for the useful references that have been included in the text.

- 345, line 10: We have clarified this point and removed the (repeated) description from page 356. The Land Model description now reads:*'... is implemented as a single "bucket" soil water reservoir. Liquid water capacity of each land cell is gathered into a single soil water reservoir, and constant values of water availability, heat capacity, roughness and drag coefficients are used. Rain can accumulate in the soil to a maximum prescribed soil moisture content (i.e., water availability). When precipitation exceeds the prescribed water capacity, a very simple basin map collects the precipitated water and idealised rivers redistribute the water back into the ocean at the nearest*

point.'.

- 346, line 8-9: Ocean model formulations use lateral and vertical diffusivity and viscosity schemes. The use of the term 'diapycnal' was inappropriate and is thus removed. The approach of mixing on, and across, constant geopotential surfaces generally results in spurious cross isopycnal mixing in z-coordinate ocean models. Thus, rotated diffusive parameterizations (the so-called neutral physics) have been implemented in most ocean climate models to properly represent along-isopycnal diffusion of tracers (e.g., the Gent and McWilliams (1990) eddy parameterization).

- 347, eqn 1: Δ_{sol} is not a function of latitude but rather a constant and in the standard case is set to 1.2 (See Table 1). Surface albedos (land and ocean) are constant at all latitudes. Δ_{sol} is indeed the parameter used to study the sensitivity of energy transports to changes in the meridional gradient of solar flux.

- 347, line 13-14: Our ocean albedo is set to 0.33 (page 346, line 16) and the land albedo to 0.45 (page 348, line 24). It is difficult to say what is the role of the 'too-high' surface albedo on the sea-ice albedo feedback. I guess a 'climate change' experiment would shed more light on the sensitivity in the present parameter settings.

- 348, line 8: Again, this is a difficult question. The basic climatology of the model agrees fairly well with the GFDL model. However, details certainly differ. For example (i) What is ENSO like in this model? (ii) How does the ITCZ look like? (iii) Is Equatorial climate variability similar to higher-end coupled models? These subjects are potentially very interesting in trying to disentangle the different role of ocean-atmosphere interactions in a model that, by construction, neglects some of the atmospheric physics.

- 351, line 5: See General Comments.

- 351, line 26: information on the code availability is now included in the Abstract.

- 353, line 5: We have changed this sentence to: *'The northern branch is vanishing for lower values of κ_v, while a more symmetrical circulation develops with increasing* *vertical diffusivity due to a stronger southern cell. The wind-driven asymmetry generated by the opening of the Drake passage will eventually become of second order importance in a diffusively-dominated circulation.'*

- 353, line 8: This statement is unjustified and was thus removed. We thank the reviewer for pointing this out.

366, table 3: We agree with the Referee. We inadvertently included those experiments in Table 3.

- 367, fig.1: the diagram is improved

- 368, fig.2: point taken. Fig. 2 has been removed.

- 371, fig.5: We have added more labels to the contours in this and following figures.

- 372, fig.6: We have added more contours, changed to 'conic' projection the sea-ice panel and moved the description from the caption to the text.

- 374, fig.8: We show now a coloured figure.

- 380, fig14; 382, fig16: See General Comments.

3) Technical Corrections: all points taken.

Interactive comment on Geosci. Model Dev. Discuss., 2, 341, 2009.