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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

Anonymous Referee #1

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General comments:

This is quite an interesting new model which plays an important role in the hierarchy of coupled models. It may be the simplest possible fully coupled model with a fully dynamical atmosphere and ocean, yet can also be expanded relatively easily into the full GFDL climate model. Care has been taken to assure flexibility of boundary conditions, and the model has been tested in a variety of physical situations, and with a range of model parameters as well. The basic results from these tests are presented in this manuscript.

I have only few comments on the manuscript, mostly involving further description of the

atmospheric component of the model and some of the justifications presented therein. These are listed below, as is a list of technical corrections.

Specific comments:

introduction: Quoting the Held 2005 BAMS paper about heirarchies of models would probably be useful as justification in this section (it also features plots from your atmospheric model!).

p.347, line 11: It's not immediately clear how a seasonal cycle should be added from the simplified form of radiation given by eqn 1: this was tuned to the annual mean TOA SW, not to the incoming solar radiation in any way. Both the seasonal or the diurnal cycle could be added into this parameterization with relative ease, but it would require making some choices.

p.347, line 14: If you added solar absorption, you would want to increase the surface albedo, not decrease it, as this sentence implies (see Frierson 2007a vs Frierson et al 2006). Otherwise the total absorbed solar radiation would increase.

p.348, line 2: "the simplified convection scheme... significantly improves the tropical climate" What is this statement based on? It's not really true in the Frierson 2007a study. It would be quite interesting to know if you have evidence that at the lower resolution used here it does lead to a better climate in some metric. This is the most computationally expensive physical parameterization by far I would think, so this statement is important to justify for other possible users.

p.348: The atmosphere is by far the most simplified part of the model. It therefore would seem important to discuss the type of studies that can be performed with such an atmospheric model. Readers would be interested to know that it's possible to use the model in parameter regimes appropriate for other planets, to use in non-hydrostatic dynamical cores, and that quantitative comparisons have been made with full GCMs. Specifically, these are the other studies that have been performed with this model:

Mitchell et al 2006 and Mitchell et al 2009: changed physical parameters to those appropriate for the atmosphere of Titan & methane condensation on this moon

Garner et al 2007: implemented in a nonhydrostatic dynamical core, made the model "hypohydrostatic"

Frierson 2007b: studied convectively coupled equatorial Kelvin waves

Frierson et al 2007b and Frierson 2008: studied width of Hadley circulation and midlatitude static stability, respectively, and compared with full GCM simulations

Kang et al 2009: studied ITCZ response to extratropical forcing, compared with full GCM

p.351, line 12: the 1 hr atmospheric time step is getting close to the standard simplified Betts-Miller relaxation time of 2 hr. Numerical stability concerns don't allow that relaxation time to approach the time step. However, Frierson 2007a showed the tropical climate is essentially insensitive to moderate changes in convective relaxation time, so you might consider mentioning that a larger value of tau_SBM could be used as default. More broadly, what limits the time step of the atmosphere? Is it set by the CFL condition at the resolution?

p.351, line 22: I'm surprised the atmosphere takes so much more CPU time compared with the ocean. Do you have the breakdown on how much of that is dynamics versus physics within the atmosphere?

p.355: The primary difference from reality I see in the model climatology is that the moisture content is significantly lower than reality (the humidity content is about 50% of that in reanalysis). This biases the energy fluxes as well (the ratio of DSE to latent energy transport in midlatitudes is about twice as much as in the observations of Trenberth and Stepaniak 2003). Is this primarily due to small humidity contents over land (or are there additionally biases in relative humidity or SST which make a significant difference to this)? It would be interesting to investigate this in a little more detail, in

particular why the Northern Hemisphere midlatitude energy transport is so much more dominated by DSE flux in this model when the fraction of land vs ocean coverage is similar to reality.

Technical corrections:

p.345, line 5: "a gray-radiation" change to "gray radiative transfer"? p.347, line 11: It's a little confusing to bring up liquid water in this paragraph about radiation. I would recommend switching this to the next bullet point. p.348, line 8: "tracer gases" change to "trace gases" or "radiatively active gases"? p.358, line 6: "papaer" Figure 6 caption: "looses" change to "loses"

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