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Geoscientific
Model Development
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 #2

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General Comments

The paper describes a new, computationally cheap atmosphere-ocean model that retains potential for much of the complex dynamics often simplified out of other fast coupled models. Although the model as described omits some of the atmospheric physics, the result is a flexible model with excellent potential for idealised climate studies; two particular examples are briefly described. The idea is a good one, the paper is generally well written and I heartily recommend publication.

Of course, I've got a few questions and comments. My most general one concerns the inclusion of brief overviews of a number of different experiments, particularly the Aqua-

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world ones. I'd rather see them written up rather more fully in a separate publication than skimmed over here - it doesn't do justice to what I'm sure are interesting experiments, and if, as suggested on page 351, the other studies may be presented more fully elsewhere and are just being "trailered" here before full publication, that seems a bit cheeky, to be honest. I realise that some idea of the model results needs to be presented along with the description, but I think that the Atlantic sector, along with the parameter sensitivity tests for Kv and/or the basin dimensions would suffice. That said, I'm all in favour of aquaplanets and things like changing the rotation rate of the Earth, so I do have some regrets about suggesting that some of this material might be better cut here.

A couple of minor, general comments: I believe Geosci.ModelDev. encourages the submission of supplementary material in the form of model code or datasets - has this been considered for the ICCM? Also, is there a version number/name that could be appended to make it clear later which iteration of the model is being described, assuming more development may occur?

Specific Comments

342.9: The use of idealised geometries is included as a saving in computational expense along with simplified physics and parameterisations. Does that imply that the geometries of the setups are hardcoded in a similar way to other model parameterisations and difficult to change? I guess my point is that I don't think of the physical geometry as a fundamental property of the model in the same way as those other things, more a detail chosen by the user for a specific experiment and not something that you'd list up front in a general model description - unless the model is limited to only certain specific options.

342.12 and elsewhere: The intended temporal scope of the model is variously described as "millennia", "multiple millennial", "inter-annual to centennial" and "annual to centennial" - this could be more consistent.

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343.2: I'm being picky, but whilst the term "most realistic" clearly depends on the use the model is put to, surely there are more "realistic" models than "IPCC-class" models? High resolution numerical weather prediction models for example - for short timescale use, of course.

343.12-18: the LSG-ECHAM3 coupled model (Mikolajewicz and Voss, Clim Dyn 16 (2000) and many since) deserves a mention here as a coupled model with a simplified ocean and dynamical atmosphere - unless I've got ECHAM3 misremebered?

343.22-29: mention could also be made of FORTE (e.g. Smith et al (2006), cited later in the ms., MOM2 with the simplified physics IGCM atmosphere for idealised studies), FAMOUS (Smith et al, GeosciModelDev. 1 (2008), also able to claim a connection to a higher-end model, HadCM3) and newer versions of GENIE (www.genie.ac.uk, which use a 7-level version of the IGCM, I think, although may not be published?). There may also be others - the MIT model used for the Marshall et al (2007) Aquaworld studies?

345.10, and elsewhere: I don't really understand the concept of a "bucket land (model) with constant water availability". Does the bucket-ness just refer to whether there's runoff or not? This description is repeated on page 348 and 356 - once is probably enough!

346.8-9 and elsewhere: vertical and diapycnal (even "vertical diapycnal diffusivity", pg344) diffusivity seem to be used interchangeably when describing the ocean model - is it not one or the other, presumably diapycnal if you've got Redi/GM? Or can you sensibly combine isopycnal and vertical tracer diffusion in a consistent fashion as suggested here?

347.eqn1: I'm confused about del_sol, if you've already got a latitude factor in mu - could you explain? Does it mimic surface albedo gradients in some way? Is this how the meridional solar flux gradient is changed in the experiment alluded to on pg 351?

347.13-14: do you have a feel for whether having generally higher surface albedoes

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affects the impact of the sea-ice albedo feedback? How much higher are your ocean surface albedoes?

348.8: the lack of clouds is interesting - with the connection to the higher-end GFDL model, can you say more on how this, or the rest of the simplified physics affects the climate?

351.5: linking with my general comment at the top, you say you're only going to talk about CTL because the rest will be presented elsewhere, and then also mention (and show figures from) the other experiments as well!

351.26: the information about availability etc. is important, and could be placed more prominently up front in the abstract or intro.

353.5: I wouldn't have said that any of the circulations in fig 8 showed real north-south symmetry, except for NDP - the north is much stronger in all of them.

353.8: I'm not clear on how your ocean resolution would cause the non-existence of the Deacon cell - could you explain? HadCM3, for example, has a comparable ocean resolution and a definite Deacon cell.

366.table3: it seems odd to list the specific values of omega you've tried with no mention at all of the experiment results. Ditto for ES - and what does that stand for?

367.fig1: do you really need all the dashed lines - they clutter the diagram, I feel

368.fig2: the information in this figure is repeated, and more usefully presented, in figures 4 and 6, making this one redundant.

371.fig5 and others: with the continuous colour scale it would be helpful to have the contour lines clearly labelled - and with more labels than seen on, e.g. fig6.

372.fig6: has colours but no colour scale, and the contour values could be more frequent. The fifth panel, for the ice, is particularly illegible - polar projections might be better? I'm not sure the climate description in the caption wouldn't be better off in the

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main text rather than here.

374.fig8: the bold clockwise contours and light anticlockwise ones are rather unclear, making the anticlockwise circulations appear, at first glance, weaker than the clockwise, even in the Kv0.05 case where they are stronger. Colours might be better?

377.fig11: with only 7 vertical levels in the atmosphere, these contour plots seem a bit misleading - especially the one for theta that shows detail of the tight temperature gradient at the top of the model?

380.fig14, 382.fig16: very pretty, but discussed even more briefly than the rest of the Aquaworld material, which is a shame.

Technical Corrections

344.14: "experiments were performed" - feels like this should be "have been", or "are" if described in the current paper

356.22: "full-Earth geomerty" - should be "geometry"

358.6: "this papaer" - should be "paper"

358.16: your Conclusions section is more of a Summary and Discussion as you don't really draw any conclusions

351.24: "would not slow down" - should be "does not", since you've tried it and you know it doesn't

372.fig6cap: "looses" - should be "loses"

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