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Interactive Comment

Interactive comment on "The CCCma third generation AGCM and its extension into the middle atmosphere" *by* J. F. Scinocca et al.

Anonymous Referee #3

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1 Summary and Recommendation

This paper describes the current (version 3) CCCma AGCM. As one of the premier models of its type in the world, a detailed discussion of its components and climate simulation issues is welcome, particularly as such descriptions have traditionally been relegated to obscure technical reports that prove hard for noninsiders to access. I also appreciated the candid discussion of practical issues like the choice of an advected moisture variable, the defense of spectral advection as a practical constituent advection algorithm given appropriate safeguards, and issues in tuning of parameterized orographic gravity wave drag. The paper is well organized and presented, and I enjoyed reading it.

I think the paper should be published after the authors have had an opportunity to consider the comments below. I leave it up to them which they feel obligated to address and which to ignore.

I disagree somewhat with the earlier reviewer's comment, in that I think both the length and scope are appropriate. My main recommendation would be to reclassify and publish it in ACP as a Technical Paper, since it seems to fit that particular type of ACP manuscript better than as a conventional research or review article: see http://www.atmospheric-chemistry-and-physics.net/submission/manuscript_types.html.

2 Section 2.2

This section is interesting and relevant for spectral AGCMs generally, and so I welcome the discussion of the CCC approach. However, I found parts of it hard to follow.

On P7888 L25 it is stated that the spectral dynamical core requires Courant numbers C less than unity. One must assume, absent any qualifying statements, that the authors refer here to the usual definition of Courant number for CFL in Eulerian global spectral models, in which the velocity term U is the sum of the maximum wind speed and largest external gravity wave phase speed. If so, this requirement of $C < 1$ implies that neither AGCM2 or AGCM3 use a semi-implicit time scheme to facilitate longer model time steps. Is this true? Perhaps not: it occurs to me the authors may be implying a different Courant number definition here involving just the maximum wind speed, which is the more relevant for advection algorithms, and given that semi-Lagrangian semi-implicit dynamical cores generally permit longer model time steps than Eulerian semi-implicit dynamical cores. Some clarification on these details is needed. Additionally, would the problems as perceived here have been alleviated or reduced if (as is more common) semi-Lagrangian was implemented as an entire dynamical core replacement, rather

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than just as an add-on advection option for dynamics driven by the existing Eulerian dynamical core (e.g., Rasch et al., 2006)?

I found the hybridization presentation starting on P7889 difficult to follow. The confusion originates in inconsistent mathematical definitions. Eq.(1) confuses since it inserts q on both the left and right hand sides of the equation (a different symbol for p should also be used since most readers will immediately assume that it refers to pressure). Eqs. (2) and (3) then present two different expressions for the same variable s that cannot both hold. It seems as if three separate symbols (say, s_1, s_2 and s_3) need to appear on the left hand sides of Eqs. (1), (2) and (3), respectively, with s_3 then replacing s in Eq. (4) to make the final connection between the actual advected variable and specific humidities clear. In addition, "hybrid transform" is a confusing term for all this, since the "transform" connotes some sort of connection to the spherical harmonic transforms associated with spectral advection of s_3 (section 3.1), possibly related to the physics grid (section 2.1). Isn't it clearer to just describe this as a different functional form for the spectrally advected moisture variable?

Since this is a model overview, it would help if the functional choices for Eqs. (1)–(3) were motivated at the start, especially since the Boer (1995) and Merryfield et al. (2003) references aren't easily accessible to the wider ACP readership. I for one can't see immediately how these functional forms are shape preserving. The more salient point in these choices seems to be an effort to reduce the dynamical range at lower humidities so that horizontal gradients are weaker and thus spectral truncation is less apt to yield negative values. Perhaps I am missing or misinterpreting key points here.

One could make the argument (presumably easily refuted?) that Eq. (6) is not much different in the end to arbitrary hole filling of advected q fields. In the stratosphere where $q \ll q_0$ you are from Eq. (3) always advecting the modified moisture variable. Since this modification reduces the dynamic range still further and requires filling via (6), do you lose something here for CMAM chemistry simulations involving middle atmosphere water vapor photochemistry and transport? More generally it would be useful to connect

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the final choices here with other previous studies of advection algorithms and moisture variable choices in global spectral models (e.g., Rasch and Williamson, 1990, 1991; Rasch et al., 2006).

3 Section 3.3

This is an interesting discussion, but left me with one large unanswered question. The warm bias in Figure 4 is a net effect of the parameterized nonorographic (S03) and orographic (SM00) gravity wave drag (GWD) in the GCM. Given that this combination yields excessive GWD-induced descent, why (P7908 L13) was it decided to extensively tune the orographic rather than the nonorographic scheme? It is stated as if obvious, but seems anything but to me. As mentioned, the orographic scheme performs well lower down and essentially switches on or off quasi-realistically in response to simulated flow patterns over subgridscale orography. By contrast, the nonorographic scheme is a statistical scheme with unrealistically uniform source distributions of GW flux that do not respond to tropospheric conditions. Given this, I would have naively thought that the more *ad hoc* nature of the nonorographic scheme demanded tuning attention first, particularly since that seems easier conceptually, as well as the known role of nonorographic GWD in alleviating AGCM stratospheric cold pole problems (Austin et al., 2003). Furthermore, retuned orographic GWD seems to come at the expense of reductions in MSLP skill. A few more justifying details would be helpful.

4 Minor Comments/Typos

P7885 L4: should the quotes around “tuning” be removed here and added to the second occurrence on L6?

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L14: While I think I understand the meaning, this first sentence is nonetheless a bit vague. Consider a rewrite.
P7886 footnote 2: typo, "Shepherdet" (sic)
P7887 L7: Is this better? *Typically, 31 vertical levels are employed with layer thicknesses that increase monotonically from ∼100 m at the surface to ∼3 km at 1 hPa.*
L17: I would urge the authors to use a different term than "reduced Gaussian grid" for their linear Gaussian grid. The term "reduced Gaussian grid" already has a specific connotation in reference to global spectral models in which the number of longitude grid points is progressively reduced as a function of latitude to maintain an approximately constant spatial resolution and save computer time (Hortal and Simmons, 1991). To avoid needless confusion, why not just "linear Gaussian grid" or "physics grid," the latter term used often later (e.g., P7890 L11)? The exact ways gridded fields on the linear and quadratic Gaussian grids interface with one another in the AGCM wasn't obvious to me, although their equivalence spectrally is quite clear.
L21: What precisely do you mean by a "1:1 spectral transform"?
P7894 L9: unresolved *mesoscale* orography? Or does this include a turbulent orographic parameterization too?
L24: delete "into each of the half spaces which lie"?
L26: ..of the waves' momentum flux continuously varies?
P7895 L2: peak vertical displacement amplitude..
L9: ..where h is peak height of the subgridscale obstacle...
P7897 L13: how are the mixing ratios of these absorbing species set?
L18: define the acronym CKD2.2
L28: *a* Padé approximation? (see also P7898 L10)
P7899 L13: "sigma" here is confusing, since your AGCM doesn't use pure sigma levels.
7901 L18: I realize this statement becomes clearer in the DYN-MAM context later in the paper when the production model with tuned nonorographic plus orographic gravity wave drag is shown to be excessively warm (Figure 4). For most ACGMs with standard

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cold-pole problems, however, isn't the issue getting the stratosphere sufficiently *warm* to avoid an unrealistic thick polar cap of PSCs over both poles?

P7904 L9–16: The chaotic downscale advection model is only relevant in the stabler stratosphere. In the troposphere isn't it well known that energetic injection at small (possibly unresolved scales) is relevant, as often conceptualized in the context of upscale-cascading mesoscale stratified turbulence?

P7905 L6: to be clear, this physics filter is being applied using the transforms on the "reduced" linear Gaussian grid discussed in section 2.1? Also, the index p should again be changed to distinguish it both from p in Eq.(1) and from pressure.

P7906 L6: These data are...

L10: How do these data deal with large diurnal variations in ozone mixing ratio at altitudes above $\sim$0.3 hPa for the terrestrial and solar radiative transfer?

7907 L1: by "seasonal mean," do you mean the 3-month averages implied by DJF and JJA? If so, that wasn't entirely clear to me.

L2: spell out SPARC.

L7909 L8: how could SM00 have discussed MSLP biases in the SM00_WMO run which was surely subsequent to the writing of the SM00 paper?

P7911 L8: "above 100 hPa" implies pressures larger than this, or altitudes below this level. Is this intended?

L18: the vertical resolution requirement pertains only to the tropics: in the extratropics horizontal resolution is much more relevant.

L28: gravity wave flux?

P7912 L20: "in a continual easterlies" doesn't make sense.

L26: define "2$\times$"

P7913 L27: I don't understand the statement that you have limited discussion to DYN-MAM. It seems to me there is lots of discussion of CMAM-only options, such as the physics filter.

7916: In this discussion it would be helpful to readers who perhaps want to try the same experiment to know the analogues of q_0 and q_{low} for radon-222 mixing ratio in

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these simulations.

P7925-7927,7929: the axis and contour labels need to be made a lot larger, especially since these plots use the descending latitude scale favored by some meteorologists. The captions here also need to quote units for the contour labels. The color scale isn't all that obvious either.

P7930: the mixing ratio units here need to be quoted.

References

[Austin et al.(2003)] Austin, J., et al.: Uncertainties and assessments of chemistry-climate models of the stratosphere, Atmos. Chem. Phys., 3, 1–27, 2003.

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[Rasch and Williamson(1990)] Rasch, P. J., Williamson, D. L.: Computational aspects of moisture transport in global models of the atmosphere, Q. J. R. Meteorol. Soc., 116, 1071–1090, 1990.

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