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Atmospheric Chemistry and Physics Discussions

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

J. F. Scinocca et al.

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We thank the reviewer for his or her comments.

While there is a need to document the details of complex climate models, it is also important to document their response properties. The response documentation, however, needs to be more that just a survey of the model output vs observations. In this paper we have attempted to provide response information as it relates to current modelling issues and in a way that might be helpful to the community.

Carefully working through the reviewer's comments, it would seem that his or her dissatisfaction stems from two main issues. The first involves the desire for increased detail in the description of some of the parameterizations. We have addressed these concerns where possible but also feel that it is sensible to point the interested reader to previous papers where much of the requested AGCM3 parameterization detail may be found.

The reviewer's second main concern involves the scientific part of the paper - particularly sections 3.3 and 3.5. The reviewer would seem to prefer that each simulation in these sections more closely correspond to observations. We have attempted to address these concerns but also we have clarified the purpose of these sensitivity tests. In particular, the value of these test do not hinge on a close correspondence to observations.

We were unaware of the new journal "Geoscientific Model Development journal of the EGU" and we note this for future reference.

A response to each of the reviewer's specific comments appears below.

1) As discussed on pg 7890 l.6-25, the source/sink nature of the hybrid transform is determined by the value of qo. The criterion use to select its value is the minimization of the correction (7) so that it is small relative to the physical tendencies of q.

2) As discussed in Section 2.8, cloud water and ice are diagnostic so transport is not relevant.

3) As discussed in Section 3.1, we have not used the hybrid approach for chemical climate modelling.

4) P.7891, L.24: Error controlled iteration is employed with a an upper bound of 50 on the number of iterations.

5) P.7891, L.24: It is the soil albedo here. The specific details regarding the snow and vegetation follow in the next two paragraphs and in the three supplied references P7891 L.5-6.

6) P.7892: Ice sheets are specified not modelled. The ice model allows for heat interchange between layers (using heat capacity of ice) and there is also snow

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melt/accumulation and sublimation/deposition along with conversion of dense snow to ice.

7) P.7892, L.17: The wording has been changed from "on the basis of measurements gleaned from the literature." to "following Verseghy, (1991)."

8) P. 7895: 2.7: The ZM scheme models water-phase clouds only. Further details are provided in Zhang and McFarlane (1995) and Scinocca and McFarlane (2004).

9) P.7897, 2.8: We have added the statement "Typically, the solar portion of the radiation is calculated every hour while the infrared portion is calculated every six hours." on P7898 at the ens of L.10.

10) P.7898, L.12: Yes, we agree and have modified the statement to say, "...permits a more accurate treatment of cloud-radiation and aerosol-radiation interactions."

11) P.7898, L.20 2.9. As described in this section (P.7898, L.21), AGCM3 does not employ a prognostic cloud-cover scheme.

12) P.7900, L.9: This was worded incorrectly. We have removed the words "zonal-mean".

13) P.7903, L.2: The statement "tropospheric sources of non-orographic gravity waves are dynamical in nature and ..." (P.7903, L.1) is a general statement that includes both resolved and unresolved dynamics. The discussion at this point is introductory. As suggested on P.7903, L.10-13 "Both orographic and non-orographic gravity-wave drag ... will be discussed in more detail in the next section."

14) P.7905: The physics filter is applied to all prognostic spectral variables entering physics and to their spectral tendencies upon exit from physics. As such it acts on all aspects of the physics package that use these fields. The main impact of the physics filter is found to be a significant reduction in the amount of mass correction due to hole filling on positive definite fields. The main prognostic variables show a relatively weak sensitive to the filter.

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On P7905 L.15 we have added the text, " When it is employed, the physics filter is taken to act on all prognostic spectral fields entering the physics package. The main impact of the physics filter is a significant reduction in the amount of mass correction associated with hole filling. The main prognostic variables show a relatively weaker sensitivity to the presence of the filter."

15) P.7906, L.28: The configuration requested by the reviewer is, in fact, already presented in Fig. 3b and would simply be repeated if included in Fig. 2.

The comparison in Fig. 2. is "simplified" because it eliminates any possible interaction between the orographic and non-orographic schemes. Further, it is insightful because it aids understanding of the relative contributions of the orographic and non-orographic schemes when they are employed simultaneously (i.e. Fig. 3). We agree that non-orographic GWD is an important aspect of the observed circulation. However, the point of the simulations in Fig. 2 was not to reproduce observations but rather to consider the middle atmospheric impact of the two different orographic schemes. The fact that the SM00 mesospheric jet core (Fig. 2f) is close to observations in the absence of non-oro GWD is interesting (L.7-10).

16) Discussion of Figure 4. The primary purpose of the WMO configuration was the modelling of polar ozone loss. As such, it was decided that the negative temperature bias displayed in Fig. 4d was acceptable. It may be that polar ozone loss could have been equally well simulated had the cold bias been smaller. We have not investigated this possibility. Polar ozone loss in the NH is more difficult to simulate from both the perspective of PSC chemistry and the temperature bias identified by the reviewer. This remains a challenge. Attempts to remove the warm bias in the NH would make the cold bias in the SH even larger. The SM00_WMO configuration represents a compromise in this regard.

In the reviewer's statement, "All panels show local maxima of positive temperature error between 150 and 100 hPa at 60S and 40N in JJA, and less strong at 50S and 50N in

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DJF. Is it known what causes this error pattern?", We believe the reviewer means 60N and 40S in DJF. The source of this bias may be related to the older ozone data set of Kita and Sumi (1986) specified in the DYN-MAM configuration of AGCM3.

17) P.7909,L.25: modified as suggested

18) The horizontal offset of the blue and red curves is approximately half a month, which is significantly more than "several days". We have modified "several weeks" to "a few weeks".

19) P.7911.,L.13/14: We proposed and were able to test one possible mechanism that could lead to a common bias among the WMO models. We do not, at this point, have another mechanism in mind about which to speculate.

20) P.7911,L.26: The reviewer's statement that "resolved wave generation in GCMs is sensitive to the tuning of parameterized processes." is entirely consistent with our discussion in Section 3.5. It is the subject of the Scinocca and McFarlane (2004) referenced in our paper. Until very recently, such tuning has been based primarily on the fidelity of the modelled tropospheric climate with little connection to the properties of resolved waves. In this sense, the spectrum of resolved waves in any GCM is a property of the the way in which the tropospheric climate was chosen to be modelled (i.e. "essentially" a property of the model). This has lead to a postiori checks of the properties of resolved waves (e.g. Horinouchi et al. 2003 referenced on p. 7911 l. 24) because modelling centres were generally unaware of the properties of these waves in their models.

21) QBO comments. The reviewer seems to have missed the point of the QBO analysis in this paper. It was to understand the impact and sensitivity of QBO like oscillations that are driven by various contributions of resolved and parameterized waves. This was controlled by the vertical resolution in the region of the QBO and by the amount of tropical launch momentum flux of the parameterized waves. The goal was not to reproduce observations. Attempting to do so would be the subject of an entire paper

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on its own.

The reviewer is correct that the QBO displayed in Fig. 6f will have a Holtan-Tan effect on the variability of the polar vortex. This has been looked at recently by a PhD student as part of his doctoral thesis and will be the subject of a separate paper. This is not a model documentation issue since the QBO presented in Fig. 6 is not currently part of the configuration of MAM used for chemical climate simulations of future climate change.

22) P. 7912, L.2: A reference (McLandress 2000) has been included in the revised manuscript.

23) P.7912,L.28: The reference Giorgetta et al. (2006) has been included in the revised manuscript

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7883, 2008.

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