

Interactive comment on “Assessing the CAM5 physics suite in the WRF-Chem model: implementation, evaluation, and resolution sensitivity” by P.-L. Ma et al.

Anonymous Referee #2

Received and published: 29 January 2014

The authors made a laudable effort to implement the CAM5 physical parameterization package, including deep and shallow convections, turbulence scheme, aerosols, cloud microphysics, and cloud fraction into the WRF-Chem. They made this physics package available to the community as an option in the WRF-Chem. This reviewer appreciates the large amount of work that has been accomplished.

The paper describes the implementation of the porting of the physics packages, its evaluation relative to CAM5 and against field campaign data in April 2008, the resolution sensitivity of the simulations, and comparison with a standard version of the WRF-MESO. While the paper presented many valuable results, this reviewer has two major concerns that need to be addressed for the paper to be formally accepted for

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publication.

Major comments:

First, the title and the abstract of the paper gave people the false impression that the WRF-Chem with the CAM5 physics package has been evaluated. What the paper actually evaluated was for a very special case at high latitude in cold season about aerosols where synoptic-scale atmospheric transport dominates but the convection schemes and the boundary scheme likely play very minor roles. It is difficult to judge from this paper whether the porting of the convection schemes is working as expected. It would be ideal if the authors can include a convective case. If not, the title and the abstract of the paper should be revised to clearly state the limitations of the paper.

My second major concern is about the lack of physical explanations on the resolution sensitivities or model differences. I understand this may be hard, but more efforts should be made to make the paper more interesting. For example, Figs. 4a and 4b showed clear resolution dependence of cloud liquid and cloud ice. The paper just mentioned the liquid “bias is reduced with increasing resolution”. It did not mention that the ice path bias is increased with increasing resolution. It did not discuss the possible physical causes. Some explanation is necessary. Likewise, the large difference in the black carbon vertical profile between CAM5 and WRF-160km in Fig.6 and possible causes should be noted. The very large difference of aerosol concentrations between WRF_10km and models all other resolutions in Fig. 9 should be discussed.

Other comments:

In the WRF-CAM5, how does the radiation code treat the fractional cloud cover? (Page 9, L26) Same as in CAM5?

In Fig.5a and 5b, why are the differences between CAM5 and WRF-160 km in BC and OM so large?

Page 19, L14, “..high BC concentration that starts around 20 April, and about one to

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three orders of magnitude when the episode starts (Fig. 7b)". The episode mentioned is not clear in Fig. 7b.

Page 20, L22, and Fig. 9, why wasn't CAM5 included in the comparison?

Page 20, L26-28, this sentence is not clear.

Page 21, L13, and Fig. 10, why wasn't CAM5 included in the comparison?

Section 6. Is the one month of April 2008 still used in the free simulation of CAM5? Is CAM5 initialized?

Page 24, L28-29: "Figure 15 shows..WRF_CAM5 ... much more realistic ...than ..WRF_MESO.." This is not obvious to the reviewer. The same comment is true for Figure 17 (Page 25, L24). A more objective measure should be used.

Interactive comment on Geosci. Model Dev. Discuss., 6, 6157, 2013.