Atmos. Chem. Phys. Discuss., 9, C1118–C1121, 2009
www.atmos-chem-phys-discuss.net/9/C1118/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Orographic cirrus in the future climate" *by* H. Joos et al.

Anonymous Referee #2

Received and published: 23 May 2009

Review of "Orographic cirrus in the future climate" by Joos, Spichtinger, and Lohmann.

General Comments A 2D cloud-resolving model is used to simulate idealized orographic cirrus clouds using thermodynamic profiles from present and future climates as predicted by the IPCC A1B simulations. A number sensitivity tests are performed to understand how dynamic and thermodynamic changes affect the microphysical properties of orographic cirrus clouds in future climate. The authors use aircraft data from the INCA field campaign to evaluate the models skill at simulating frequency distributions of vertical velocity, ice number concentration, and ice water content in orographic cirrus clouds.

Few global models represent orographic cirrus cloud formation, which tends to be forced by processes that are sub-grid with respect to the GCM grid box size. Orographic cirrus will likely have only a regional impact in areas that are downwind from

mountain ranges. Nonetheless, exploring the effect of a warming climate on orographic cirrus may have some implications on regional climate, although this is not well motivated by the authors.

Overall, I find that this manuscript is well organized and well written. The authors analysis is very thorough and well thought out. The conclusions are well supported by the sensitivity studies and figures. I would recommend this manuscript for publication in Atmospheric Chemistry and Physics after minor revisions are made, as detailed below.

Specific Comments

1. It is unclear to me how changes in the large-scale circulation in future climate feeds back to the dynamic flow regime. As I understand, your cloud resolving model simulations are initialized using the future climate thermodynamic profiles from IPCC simulations. Do your simulation results in Fig. 9 include the effect of the large-scale circulation, or are those changes only included based on the initial thermodynamic profiles for the 2090-2099 simulation? Please clarify this in your discussion of Fig. 9 and the simulation set up.

2. One of your conclusions is that the IWC responds more to thermodynamic changes than to dynamic changes. However, if dynamic flow regime does change (as in the North America simulations) then dynamic regime will contribute to changes in IWC. This should be summarized/emphasized in the abstract and conclusions.

3. Sec. 2, first paragraph: In your discussion of the microphysics schemes in the EULAG model, it is unclear to me if you used standard options, or if you implemented new schemes. Please be specific about what is standard in the model and what is your new feature.

4. The model simulations are verified (suggest evaluate rather than verify) using FSSP-300 measurements. Measurements of number concentration in ice clouds using FSSP measurements are now believed to be overestimated due to shattering effects (see

recent papers by Field, McFarquhar and others). You do not mention this possibility in your discussion of model-data comparisons in Sec. 3. In particular, your particle size distributions (Fig. 3, middle plot) shows number concentrations between 1 and 10 cmˆ-1. These are quite large. Supposing that shattering was a problem, what effect would this have on your evaluation and conclusions? You should add some discussion justifying your results in regards to this topic.

5. Sec. 4.1.2, lines 8-13: I am a little confused by your comparisons here. Are you saying that you are adjusting the time to account for the variation in onset of cloud formation? Or that you don't need to because it doesn't make a difference in Fig. 6? I think that these statements need to be clarified a little bit to stat clearly what is shown in Fig. 6.

Also on P. 8954: line 13, do you mean T=220 K instead of 20 K? and Line 24-27, P. 8954, "Therefore the reduction in ICNC... resulting in tau for th warm case is slightly lower..." This statement does not match Fig. 6, where I see in the left column tau_warm is less than tau_cold. Please clarify your statements in this paragraph.

6. P. 8961, lines 9-14: You state that an increase in IWP leads to a reduction in ICNC etc. but you do not discuss the physical mechanisms behind these changes. For example, IWP does not cause a decrease in ICNC, but if fewer ice crystals form, then crystals will grow larger/faster, and hence IWP will be larger. It seems that you have the cause and effect backwards. Please supply a better description of the physical mechanisms driving these changes (rather than temperature changes, hence the microphysics change).

7. Sec. 5.3, Fig. 11: In Fig. 11, it seems that in the future climate simulations (right panels) the gravity waves are dampened downstream.

8. Conclusions, P. 8967, Line 13: How robust is the assumption that the relative humidity remains constant in the cloud during future climate simulations? This assumption would have significant impacts on your simulations in both the northern and southern

hemisphere. This seems to be a major assumption in your analysis and should be thoroughly justified.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8943, 2009.