Atmos. Chem. Phys. Discuss., 9, C4515–C4518, 2009
www.atmos-chem-phys-discuss.net/9/C4515/2009/


Atmospheric Chemistry and Physics Discussions

ACPD

9, C4515–C4518, 2009

Interactive Comment

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

**H. Joos et al.**

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Received and published: 4 September 2009

We thank the reviewers for their helpful comments.

Response to reviewer 2:

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.

Response: The changes in the large-scale circulation have been taken into account by using the vertical wind profiles for the corresponding region and season for the present and future climate. See figure 8, fourth column. However, we only took into account changes in wind speed and neglected the changes of the wind direction, which are quite small. As only idealized simulations are performed, it would not make sense to take this effect into account here, as we would need a 3-dimensional setup with realistic topography to investigate that.

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.

Response: We added an additional sentence in the abstract and summary and discussion sections in order to emphasize the importance of changes in the dynamical flow regime.

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.

Response: We used the standard version of the model, which contains a detailed two-moment ice microphysics (Spichtinger and Gierens, 2009a). We did not implement new features. We clarified that in the beginning of section 2.

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 cmËĘ-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.

Response: In the discussion of the measurement campaign (INCA) Gayet et al. (2006) state that for this case shattering is not an issue, i.e. high ice crystal number concentrations of small ice crystals are real. Their conclusion is based on the fact that they used different techniques to measure the ice crystal number concentration. As the comparison of these results showed little difference, they state that shattering was not important. We added the reference and remarks in section 2.3.

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.

Response: The onset of cloud formation is delayed for RHi=110%. As the pictures we show all refer to t=5h, they do not show the same stage of development of the cloud. However, we also looked at the pictures at the same time after the first nucleation event (e.g. 3h after the first nucleation), but the overall picture did not change. We added this explanation to the text.

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.

Response: This paragraph was confusing. We rewrote 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



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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).

Response: This sentence was written in a misleading way. We wanted to say that the reason for the increase in IWP is the assumption of a constant relative humidity with increasing temperatures. On the other hand, increasing temperatures lead to a faster growth of the crystals, such that the supersaturation is depleted faster and no new crystals can form. We added that explanation to the text.

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.

Response: This effect is probably due to the changed stability.

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.

Response: Yes, that assumption is very important for our study. We refer to Held and Soden, 2000 where they show, that the increase in water vapour concentrations in a future climate which is simulated is similar to an increase which is simulated if a constant relative humidity is assumed. We included this explanation in section 5.1

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



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