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Atmospheric Chemistry and Physics Discussions

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

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We thank the reviewers for their helpful comments.

Response to reviewer 1:

My only major complaint is that there needs to be some clarification of the effect of moisture on vertical wavelengths in the model simulations. The impression given in the paper is that moisture affects the vertical wavelength at all altitudes (based on using the equivalent potential temperature instead of the potential temperature to calculate stability). In fact, the stability changes only at altitudes where condensation occurs. At most altitudes there is no condensation, and the atmosphere should behave like a dry atmosphere (excepting the "virtual temperature" effect – not included here). It is not clear what assumptions the authors have made on this. This is of little importance for the discussion of the Southern Hemisphere. However, it appears to matter quite a bit

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Interactive Discussion

Discussion Paper

for the Northern Hemisphere discussion. How much of the instability problem at low altitudes in summer in NH is due to using equivalent potential temperature instead of potential temperature? If, indeed, the authors are assuming that condensation occurs everywhere, there needs to be some justification of this assumption.

Response: Thank you for this remark. In the simulations presented, we assumed condensation to occur everywhere (in the whole vertical column) by taking the equivalent potential temperature as input profiles. Of course, this represents an extreme case and might not be realistic. Therefore we repeated all “moist” simulations shown here by initializing the model additionally with the potential temperature in order to completely neglect the influence of moisture on the flow regime in the low levels where no clouds form. The results of these dry simulations are briefly discussed after the detailed description of the corresponding moist simulations and the results of all dry simulations are additionally summarized in table 2. However, both setups represents “extreme cases”, the realizations in a future climate are between the two extremes.

The authors use "mean" profiles to do their calculations. This is reasonable, since it puts some bounds on the problem. However, there is enormous variance (day-to-day) in the conditions that generate mountain waves. Since the process generating the clouds (and even, in some cases, the waves) is horrendously nonlinear, it is possible that some systematic behavior in the variance might be comparable to changes due to the means. Obviously, there is a difference between: (1) the change (from current to future climate) in the mean optical depth of all orographic waves and (2) the change in the optical depth of orographic clouds generated by the mean flows. An example of this might be the summer case (SH), where there is almost no cloud for the "current" climate. Essentially there is a cutoff for the mean situation because temperatures are too cold and wave amplitudes too weak. The results presented overstate the climate effect, since if variance in the meteorology is taken into account there will be plenty of cases (in the current climate in summer) where the nucleation threshold is reached and ice crystals form. Note that for the non-linear case, summer optical depths are larger

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than in winter, again showing the large effect of changing dynamics.

Response: Of course the use of mean profiles might cause problems especially as the variability of the meteorological conditions cannot be taken into account. However, in order to set limits to the simulations we were not able to look at the day-to-day variability. We added a statement like you proposed, that using the mean profiles could lead to an overestimation of the effect of climate change and that taking day-to-day variability into account would probably lead to many cases where the nucleation threshold is exceeded. (See lines 405 ff) An additional remark has been included in the Summary and discussion section.

The sensitivity to the wave phase is an interesting point. I think it demonstrates the large variance in cloud properties that can be expected. One wonders whether systematic behavior in these variances can swamp the signal from the changes in clouds from changes in mean dynamics, temperature, and moisture.

Response: The position of the ISSR in the vertical wave phase strongly influences the microphysical properties and these changes might in some cases be more important than changes due to climate change. However, it would be too speculative to make detailed statements about this behaviour here.

I am confused by the full paragraph on page 8954. I don't see any evidence of a reduction of optical depth with increasing temperature for the 130% supersaturation case. The authors say that "This example points out, that if the increase in temperature is strong enough and the initial ice supersaturation is high, the increase in tau due to more IWP is not the dominant effect anymore as the cloud ice is reduced due to sedimentation and the optical depth is reduced." Only at the lowest supersaturation do we see a decrease in optical depth with temperature in Figure 6. Please clarify. The typo on line 13 (T=20K) helped throw me off.

Response: We changed this paragraph.

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Minor comments: In Figure 9 and 8, label A1B9 and A1B0. Response: done.

In a number of cases "extend" (verb) is mistakenly used instead of "extent." (noun). Response: Changed

Figure 11 and 9 have the positions of the A1B0 and A1B9 plots switched. There are a lot of cases. Consistency between figures will help. Response: Changed

Grammatical/typographical issues: Response: all corrected.

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

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