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Atmospheric Chemistry and Physics Discussions

ACPD

6, S5652–S5655, 2006

Interactive Comment

Interactive comment on "The global impact of supersaturation in a coupled chemistry-climate model" *by* A. Gettelman and D. E. Kinnison

Anonymous Referee #1

Received and published: 22 December 2006

Overview:

The paper addresses the effects of ice supersaturation on stratospheric chemistry and tropospheric climate in a coupled climate-chemistry model. Although the representation of the new cloud formation scheme is not the optimum when judged against supersaturation values actually found in the atmosphere and against the physics of ice nucleation, the results are mostly plausible and interesting. In particular, the paper shows the far reaching consequences of a neglect of ice supersaturation in most current large-scale models, even when the model modification tested here is only modest. Overall, I think this paper should be published after some revisions as outlined below.

Major comments:

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1) Section 2.2, 1st par: I cannot understand the description. For instance it is not clear whether the changes also apply to mixed phase clouds or even to water clouds. Also the choice of the upper RHi limit of 120given by Koop et al. The par. should be reformulated and the low threshold should be justified.

2) Sect. 3.1, 2nd par: I don't understand why the ice mixing ratio decreases in the same way as the cloud fraction decreases, and I don't see the consistency stated by the authors. It may be a consequence of the ice physics implemented in the model, this should then be explained. The fact that the precipitable water (water column) decreases by only 10% contradicts the statement that the ice water content decreases by 50%.

3) Section 3.1, 1st par: I wonder why cirrus clouds are classified as clouds with $\tau < 3$. Do you want to distinguish cirrus clouds formed by slow uplift from cumulonimbus anvils? Also, it would be nice if you could show the effects separately for subvisible ($\tau < 0.03$), thin (0.03-0.3), and thicker cirrus ($\tau > 0.3$).

4) Section 3.1, 3rd par: It should be checked whether this small change in precipitation is statistically significant. If yes, could it also be a consequence of not restrictiong the SSAT scheme to low temperatures?

5) Section 3.1, 4th par: How robust is the 0.6 W/m^2? If you estimate the errors of the single forcings, you probably end up with 0.6 ± 1.2 or so, so that perhaps even the sign of the effect is unclear. Please check. This comment also refers to the respective discussion in section 4.1

6) Section 3.3, 2nd but last sentence: Unclear meaning. Reduction of cloudiness enhaces OLR, ok. But how can this be the cause for the warm temperatures? Further above in this section the authors explain correctly that reduction of cloudiness reduces local absorption, hence cooling should result. Please clarify.

Minor comments and simple corrections:

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1) Generally keep condensation and freezing apart. The text often says condensation where freezing is meant, e.g. in the introduction, 2nd par.

2) Introduction, 1st sentence: vapour pressure is the H2O partial pressure in the air, it is NOT the vapour pressure of water or ice (there is a vapour pressure even when there is no water or ice), so the words "of water or ice" should be deleted.

3) Introduction, 1st par, last sentence: the words "at low temperatures" are superfluous and should be deleted.

4) Introduction, 2nd par, 1st sentence: The sense of this sentence is not clear. Supersaturation is an aspect of the state of the atmosphere; it is IN there. Perhaps the sense becomes clearer when the word "model" is inserted before "atmosphere".

5) Introduction, 2nd par, last sentence: What is meant here? When I understand Koop's theory correctly, the freezing threshold depends on temperature and NOT on the chemical nature of the aerosol.

6) Introduction, last sentence: check LaTex formatting.

7) Page 12437, line 22: a) "56 species"; b) it is not the species that represent the processes, hence the sentence should be slightly improved, e.g. "and represents" instead "representing".

8) Page 12442, line 20: Better sentence: The minimum in water vapour occurs 1-2 months later in the simulation than in observations.

9) Section 3.4: A formula defining TEM should be provided.

10) Page 12445, 1st par: the constructions like "less (negative) downwelling" etc. are not useful for understanding. I am puzzled.

11) Page 12445, line 24: brackets should be deleted.

12) Page 12446, lines 13-14: bad sentence "One of the difficulties is .... is difficult."

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13) Page 12448, lines 17 and 18: "effect".

14) Page 12449, line 3: delete "is".

15) Page 12449, line 12: "integrating several column runs in latitude": meaning unclear.

16) Page 12449, line 15: delete "that".

17) Section 4.1, 2nd par: I think you cannot state that a change is linear from having just two points. You can only say that the resulting change of stratospheric H2O is of similar size as the prescribed change of allowed supersaturation. (See also abstract).

18) Page 12451, line 13: "an" internally...

16) Page 12452, line 2: what is the "future trajectory of the stratosphere"?

17) Figures are generally too small, it is difficult to distinguish positive from negative differences, and the labels on the isolines are difficult to read. Units should be checked: for instance it says in figure 2 "differences (supersaturation - base)", but the units are percent, even for the specific humidity. The reader does not know, whether (supersat-base)/base is rather meant. The same refers to Fig 6. Furthermore, in Fig 6. what happens when w^*(base) is close to zero? The negative maxima in 6b) are exactly where w^* is very small in 6a). The same remark is true for Fig. 7, which is a mess anyway. Please think about a better representation.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12433, 2006.