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Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #2**

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General Comments

The paper is mostly clearly written and gives a thorough investigation into the many potential consequences of allowing supersaturation in a climate model, including the impact on the ozone and radiative fields, in addition to the more obvious fields of temperature, humidity, cloud ice and cloud cover. The paper can be published with minor but necessary revisions. My main concern relates to the parametrization of supersaturation itself, and its description in the paper, which is very cursory. Although the article makes pains to stress that the scheme is simple in construction, this can not be an excuse for the dearth of information concerning the scheme. The sensitivity study is interesting enough to warrant publication, and the caveats of simplicity are duly noted, but the issues below deserve further attention.

Specific Comments

My comments therefore relate to the text of section 2, pages 12437-39.

1) There is no mention of the ice generating processes, or how these are modified with the new scheme. There is only a single reference to Rasch and Kristjansson, 1998. One of the characteristics of ice nucleation is that, although it occurs at a threshold relative humidity (RH) significantly exceeding 100%, once nucleation has occurred, the depositional growth can reduce in-cloud RH almost back to 100%. Is this taken into account in the new scheme? Or does the new scheme simply increase the saturation mixing ration by 20%. This is an important aspect, since if the threshold is simply increased, then the RH within clouds will stay at 120%. This would mean that the relative humidity would be overestimated, and the ice amount significantly underestimated. These assumptions should be clearly stated.

2) There is also no attempt to justify the thresholds used in the scheme. Why does full cloud occur at RH=120%, when this is much below the typical upper tropospheric threshold for the onset of nucleation. I rather suspect that this is related to point 1. Since the scheme leaves RH elevated in cloud (I think), then using a higher threshold probably led to an exaggerated climate effect. Nevertheless, the threshold chosen needs to be justified. Why was a 110% threshold not chosen? or 130%? Otherwise, one can not trust the magnitude of the sensitivity, only the sign.

3) Related to point 2, why is the diagnostic estimate of the subgrid-scale variance changed between the BASE and SSAT experiments? In the Base experiment, the clouds form at RH=90% and then the gridbox becomes overcast at RH=100%. In the SSAT case, the these figures are 100% and 120%, respectively. In other words the estimate of the water vapour subgrid-scale variance is doubled in the SSAT case. Why not keep this estimate fixed to have a cleaner set of experiments.

4) Validation: The paper is quite comprehensive in its examination of the climate sensitivity to these changes. However, no attempt to validate the scheme itself has been



ACPD

6, S5431–S5433, 2006

made. One very easy test would be to examine the normalized PDF of RH in the upper tropopause and compare this to the distribution laws observed in the Mozaic data by Gierens et al. 1999. Access to the observations themselves in not required, as Gierens et al. provided a very good empirical law that fits the data well. I worry that the new scheme does not model the PDF well, and will simply shift the control case PDF to the right, with an absolute cut off at RH=120%. This means that the scheme will overestimate the incidence of RH in the RH=100 to 120% range, and of course drastically underestimate the incidence of RH exceeding 120%.

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