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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

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We are preparing a revised manuscript to answer the concerns of the reviewer and of all reviewers. Below we make some general points, and then respond directly to the reviwer's concerns. The replies discuss changes we will make to a revised version of the manuscript which we will send to the editor.

In general, we agree that we should better describe the supersaturation scheme, despite this being a sensitivity study. We have spent some time and rewritten our description of the supersaturation scheme, including adding an additional figure that illustrates the performance of the scheme relative to recently published observations of relative humidity and supersaturation. This was a point raised by several of the reviewers, and we acknowledge it could have been clearer. There were also one or two mistakes in the description (such as the thresholds for condensation) that we have corrected.

In addition, we highlight that this really is a sensitivity study, and not a detailed treatment of supersaturation, which is beyond the scope of this paper. We are attempting a sensitivity study to look at the chemical, dynamical and radiative effects of supersaturation, not a detailed physical study of how supersaturation should be properly represented. We will highlight this better in the revised text to avoid confusion

We have further made changes to the manuscript to clarify various points raised by the reviewers. These points are valuable for clarifying several confusing points, and we thank the reviewers for their time and effort.

Specific Replies to the Reviewer:

As noted above, we have significantly modified the description of the scheme to clarify it as requested and detailed below, as well as adding a figure which illustrates the scheme compared to observations and the base case.

W note that much of what reviewer 2 is asking for is beyond the scope of this paper. We are attempting a sensitivity study to look at the chemical, dynamical and radiative effects of supersaturation, not a detailed physical study of how supersaturation should be properly represented.

There is little detail in description of the scheme because the intent of the changes was not to model supersaturation, but rather to model the impact of imposing supersaturation. It is not realistic to model the processes that create supersaturation appropriately in the global model, so we have chosen a more ad-hoc approach.

Our motivation with this approach is to simulate the effects of supersaturation (more stratospheric water vapor and fewer ice clouds) in a manner consistent with the model physics. We are not trying to model the ice nucleation process or to physically represent supersaturation or the observed distribution of supersaturation.

We have also tried to make this intent clearer throughout the paper.

Specific questions:

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1. There really is no 'ice generating process' there is simply the bulk conversion of vapor to condensate given environmental thresholds. The microphysics reduces to rates which perform this conversion, and are fairly crude. We have noted this in the text.

2. We have not tried to justify the thresholds because they are not really justifiable. The work is designed as a sensitivity experiment. A proper treatment of nucleation would be preferable, but rely heavily on parametrized sub-grid scale thresholds which are probably not appropriate for use with this microphysics scheme. We have noted this further in the revised text, and we have added some justification and evaluation (see below).

3. And regarding the sub-grid variability issue, the text is wrong, and we have not changed the sub-grid variability: the threshold for condensation for pure ice is actually 110\% RH (not 100\% as stated in the submitted text). We apologize for this error and it has been corrected.

4. We have now presented an evaluation of the scheme against observations. We have discussed these points better and show some validation of the scheme by comparison to observations as suggested. A new figure in the revised version shows probability distribution functions of RH from the base case, the supersaturated case, and observations of relative humidity from the Atmospheric Infrared Sounder (AIRS) which clearly shows the differences and will help us discuss them.

We hope this revised version will address the reviewers' concerns and the reviewer will accept the spirit in which we are writing this paper. The points are all very good ones, and we hope to address these issues with further development of the model physics (which is currently in the planning stages).

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

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Interactive Comment