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Atmospheric Chemistry and Physics Discussions

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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 #3

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Gettleman ACP 06 review

General comments: This is overall an interesting study that is well presented. It is certainly only a first step in this direction, but should provide useful guidance to future efforts. I recommend publication following revisions. I have suggested several clarification, a few places where further insight would be very helpful to those trying to use this work as a guide later on, and a couple places where the paper should be shortened. I hope the authors find these comments useful.

P12434, L3: Are the authors certain that 'most' GCMs don't have supersaturation? For the few that I know about, some do and some don't. Perhaps 'many' would be better here.

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P12434, L9: The authors claim that stratospheric water increases ‘nearly linearly’ with supersaturation. To be honest, they have only a single data point. While for their one experiment the response was quasi-linear, it doesn’t follow that this is always true.

P12436, L2: Along with the Stuber et al, Joshi et al, and Forster and Shine papers, the authors should cite Shindell, GRL, 2001 here.

P12436, L18: The authors state here that supersaturation is ‘critically’ important. The paper is trying to assess this, so its certainly premature to draw this conclusion in the introduction.

P12438, L20: The authors should provide a more thorough description here of how the cloud fraction relates to the supersaturation parameterization in a general sense. This starts at 100% RH and extends until 120% RH, but its not clear exactly how the cloud fraction is affected. It there a linear dependence of the cloud fraction on supersaturation over this RH range, or is there a random nucleation process for clouds where the mean seeding follows the RH, or what happens at least qualitatively? A more thorough description of how the supersaturation parameterization interacts with the cloud scheme in general is warranted.

P12439, L19 & 20: ‘analyzes’ should be ‘analyses’.

P12440, L15: ‘above 600 hPa’ is bad terminology. ‘Above’ means ‘higher up’ to most people, but could also mean a greater pressure. Please switch to ‘greater than’.

P12440, L20: In this case, ‘highest’ in the tropics is ambiguous and could refer to the largest cloud fraction or the highest altitude. Please switch to ‘largest’.

P12441, L5: The authors should simply calculate the lifetime. They give the change in tropical precipitable water, but if they look at global and then see that the total precipitation has increased by 4%, this will tell if the lifetime has changed. However, none of this really answers the question of why the delay in condensation would change the lifetime.

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P12444, L12: It's plausible that the tropical high cloud changes indeed drive the temperature and water changes. However, it's not at all clear why the cloud response is so much larger in boreal summer. Could this be a sensitivity to the base climatological temperature? I believe it's important to look into this further as this seems to determine the sign of the overall response. Thus to eventually evaluate models, we need to know where this strong seasonal dependence of the cloud response comes from (i.e. what do we have to be doing right to get the proper cloud response?).

P12444 & 12445: Section 3.4 concerns the change in circulation. I find the discussion of cause and effect somewhat unclear here. The section seems to argue that the circulation changes cause the temperature changes. But then it's not clear what's causing the circulation changes themselves. The primary change identified in the stratosphere is the temperature change due to water vapor changes there, so perhaps its not so clear that the circulation drives the temperature. This would require something like a change in forcing from the troposphere, which is not obviously taking place. I hope the authors can clarify this discussion.

P12446-8: Section 3.5 discusses the changes in ozone and other species at some length. The species other than ozone are introduced primarily to account for the ozone changes. However, the conclusion of this section is that the ozone changes outside the polar lower stratosphere are very small and generally not significant. I recommend that from the last paragraph on P 12446 though to P12448, L4 all be deleted, along with associated figures. This level of detail is really not necessary given the end result.

P12449: Section 4.1 'Discussion'. Except for the first and last paragraphs of this section, the rest is a summary with minimal new discussion. I recommend the authors greatly shorten or even drop this whole summary. Concise papers are much more pleasant to read.

P12452, L7-18: I'd like to simply state that I found this section particularly interesting.

P12453, L3: The authors note that the inclusion of supersaturation leads to a better

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annual cycle, but other issues as well. If I understood correctly, it also worsened the overall positive bias of water in the stratosphere, no?

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