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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uncertainties in atmospheric chemistry modelling due to convection and scavenging parameterisations – Part 1: Implications for global modelling" *by* H. Tost et al.

Anonymous Referee #3

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Review of: Uncertainties in atmospheric chemistry modeling due to convection and scavenging parameterisations – Part 1: Implications for global modeling by H. Tost et al.

The paper gives in great detail the impact of 5 different convective parameterizations on a large number of chemical tracers. This paper serves to remind us that differences in convective parameterizations may have a large impact on chemical simulations for a wide number of species.

This paper contains some interesting material, but in my view considerable work is necessary before it can be published. My primary concerns are: that the approach taken in the paper is not unique, a similar approach was taken almost 15 years ago (point 1 below); it is not clear how robust the results of the study are (points 2 and 3 below); in many cases the paper does not analyze in detail the physics behind the differences in the convective schemes (point 4); the paper is very long so that any important information content is buried (point 5). In light of these concerns I am not sure what the goals of the paper are or its contribution.

Major Comments:

1) One of the main motivations for the paper is that "in contrast to previous studies" this paper examines the impact of different convective parameterizations on the transport of species. However, a similar study (Cumulus parameterizations in chemical transport models, JGR, 1995, Mahowald et al.) examined many of these same points almost 15 years ago (M. Lawrence was in fact acknowledged in that study). That study examined 9 different parameterization schemes including some of those referenced in this present paper. It also has the advantage that it compared results with observations. While it only analyzed simple tracers the approach taken in the present paper is not unique. The study by Mahowald et al. should not only be referenced, but the present study needs to rethink its objectives in light of the previous work. One of the justifications for this study (that this has not been tried before) is simply not valid. The other important conclusion in this study that convective transport impacts short-lived species more than long-lived species is also not new.

2) One of the main conclusions I might draw from the present study is that simulations of a wide variety of species are sensitive to the parameterized convection. This is not surprising nor particularly new (e.g., the study by Mahowald et al.). However, it seems that by tuning the precipitation and OLR, the present study wants to make the point that this is an uncertainty we may have to live with. However, I'm not sure the paper supports this. The authors state it is outside the goal of this study to examine the

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validity of the various parameterizations. Are these parameterizations all equally valid, or are some simply bad and should be disregarded? At a minimum the authors should include a table showing bias and correlation between model calculations and observed precipitation and OLR fields. A better approach might be combine the results here with a more observational paper (part 2?).

3) The authors go into considerable detail giving differences between the resulting transport for a number of chemical constituents. However, it is unclear how robust these differences are. Each scheme has been tuned to get the correct precipitation and OLR – but clearly this tuning will depend in part on the boundary layer scheme used and the cloud scheme. The tuning might be completely different with a different cloud scheme, or boundary layer scheme. Therefore it is not clear to me that the differences between the convective schemes will carry over to other simulations or model setups. Another source of concern is that only four months of meteorology is used. To what extent are the results applicable to other seasons?

4) In many cases the authors present a rather superficial analysis of why the schemes differ. In many cases the analysis is based on educated guesses, in others the authors admit that they cannot explain the differences. While an explanation is indeed difficult, a more indepth analysis might have proved interesting.

5) In light of points (1)-(5) the amount of detail concerning the differences in the schemes seems unwarranted.

Minor Comments:

1) Very little is said about the lightning distribution. However, this distribution is usually sensitive to cloud height so that very small differences in the parameterizations may significantly impact the lightning. While global lightning amount is evidently tuned between the various simulations I expect there may be large regional differences and large chemical impacts. This impact should be discussed and evaluated when comparing the different schemes.

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2) The convective parameterization, precipitation and radiation interact with a large number of other parameterizations including boundary layer parameterizations, shallow convective parameterizations, microphysical parameterizations and cloud parameterizations. These parameterizations should be listed in Table 1.

3) Page 11007, line 16-18: “Even though. . ..”. I’m not sure I really understand this sentence. What do you mean by ‘global modeling’? Many numerical weather predictions are global and involve modeling.

4) Equation (2). It seems the mixing ratio should also be impacted by entraiment into the cloud, but there is no term which depends on entrainment.

5) Equation (2). “unaffected by convection”: this is misleading as the 1st term is clearly impacted by convection.

6) It is not clear how the “tuning” was accomplished. Did the authors try to minimize the errors in any systematic way, or was this a quick tuning by hand. Some more detail here is needed.

7) The abbreviations for the different schemes used are only given in the table. This makes the paper somewhat difficult to read, especially as the abbreviations do not match the figure caption titles.

8) Page 11013, line 10: It is not clear how the Hack scheme relates to ZHW? Is Hack only run in conjunction with ZHW?

9) Page 11013, line 3: It is not clear why the overturning time is shorter in simulations with smaller mass fluxes. This would seem to imply that zero mass flux would imply the fastest overturning.

10) Page 11016, line 11: “too much” – do you mean in comparison to observations?

11) Page 11016, line 18: As stated above a summary table of evaluations should be given in this paper.

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12) Page 11020 and 11021: This explanation is not really that clear. Is the explanation largely conjecture, or have the authors analyzed this in detail?

13) Page 11022, line 11: "exemplary species" – not sure what you mean here.

14) Page 11024: This seems to imply that differences in convection may impact the large scale circulation. This is an important point and should be discusses at the outset of the paper.

15) Page 11025, line 19: "lower hemisphere"?

16) Page 11026, line 2-7: This paragraph needs some rewording.

17) It would be helpful to examine differences in nitrate and sulfate deposition in terms of their lifetime.

18) Page 11029, line 16-22: I don't follow the argument here.

19) Page 11030, line 23: "Decreasing the amount of condensed water decreases the moist static energy". This is not clear to me. Decreasing the condensed water should increase the specific humidity and thus increase the moist static energy.

20) Page 11031, line 4-10, "However..." This is a rather confusing sentence. Motivations for the study should probably precede the results.

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