

Interactive comment on “Tropical deep convection and its impact on composition in global and mesoscale models – Part 2: Tracer transport” by C. R. Hoyle et al.

Anonymous Referee #2

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The article by Hoyle et al. presents an intercomparison of large-scale chemistry transport model simulations within the framework of the SCOUT-O3 project to examine transport of idealized tracers in tropical deep convection regions. Convective transport of tracers in large-scale models continues to be a poorly-constructed parameterization, thus needing further investigations and assessments. This paper illustrates the similarities and differences of various chemistry transport models for tracer transport by tropical convection. In addition, there is some comparison to measurements to guide the evaluation of the modeled, idealized tracer transport.

The topic of the paper is appropriate for Atmospheric Chemistry and Physics. However,

C10009

the paper needs to be substantially revised before publication. As I was asked to review this paper after other reviews for both this paper and Part 1 were submitted, I agree with many, if not all, of the comments of the first reviewer. I was able to find the Part 1 paper, and have also scanned some of the comments for Part 1. My overall suggestion is to focus on the main points of Part 1 and Part 2, and do not clutter our thinking with details not important to these main points.

Major Points

1. Much of the paper provides weak ascertains (e.g. “Some of the offset of the models towards higher CO values may be due to the idealised CO tracer having a longer lifetime than CO in the atmosphere” in Section 4.4 and “The timing of the transport events is probably of lesser importance for average tracer mixing ratios” in Section 5) that need to be strengthened with supporting evidence. On the other hand, the abstract does a fairly nice job of stating what the findings of the study are. The same conviction expressed in the Abstract should be used in the body of the manuscript.
2. In the Model Description text, the models should be described in a consistent fashion. I find that it is much more useful to include a brief description or terminology of a method than just providing a reference. In Table 1, the convective parameterization used in each model should be included; less important is the reference which can be included in the text.
3. This paper should be able to stand on its own, without the need to have Part 1. Because of this, there is a need to incorporate more information into Part 2. For example, a plot showing the regions of study would help greatly. A summary of the results of Part 1 would provide necessary background information. Specifically, section 4.1 says what Part 1 addresses (compare modeled and observed cloud top height distributions to investigate the capabilities of convective parameterizations), but does not say what the conclusions are. Certainly these results will relate to the tracer transport results!

C10010

4. In the Results section, there are many places where explanations of differences between models are not given. Gaining insight as to the causes of these differences would guide the community as to ways to improve parameterizations in the large-scale models. Specific issues are included in the next section.

Specific Points or Questions

1. Title: As Reviewer 1 suggests, the title should be changed because the composition of the troposphere is not addressed. Idealized tracer transport is the topic. Why not use a title that describes only what is discussed in the paper, e.g. Intercomparison of tracer transport in large-scale chemistry transport models for tropical deep convection regions. (In my opinion, the regional-scale models are behaving much like a global model but with the addition of lateral boundaries.)

2. Abstract: While I applaud the presentation of the Abstract, I am concerned about over-stating some results. For example, the importance of the boundary layer scheme was based on only one comparison with another PBL scheme. However, there are many PBL schemes that were not tested. How do the authors know that "...tracer profiles are found to be strongly influenced by ... boundary layer mixing parameterisations..." when it could be that the Louis (1979) scheme is the anomaly? In other words, I am not sure the paper provides enough evidence to give such a broad conclusion.

Think carefully about each conclusion stated and whether there is strong evidence to support that conclusion. If the authors think that it is not the role of this paper to conduct further analysis on a topic, then the conclusion must be stated as "this study indicates a process is important and should be further investigated in more detail".

3. Section 2.2 KASIMA CTM. Besides including a reference to the model, an explanation of how the lower boundary (at $z = 4$ km) is handled would be useful.

C10011

4. Section 2.9, CATT BRAMS: Nothing is said about the advection scheme. Again, it is better to describe the type of scheme (e.g. semi-Lagrangian, flux-form, monotonic, positive definite) than to just give a reference.

5. Section 2.10, WRF NWP: There are a couple of options for the type of advection in WRF. The first is not necessarily conserving and is not positive definite, while the second is positive definite. Thus, it is possible that WRF results have numerical issues with advecting tracers. Please elaborate as to which advection scheme is used.

6. For the regional models, how are the lateral boundaries handled (both for meteorology variables and tracers)?

7. Section 3.1, T6h description: Is the tracer continuously replenished in the $z < 500$ m region, so that $T6h = 1$ at every time step for $z < 500$ m?

8. Section 4.1, first paragraph. First, the paragraph is poorly constructed. The background information should come before the Figure is introduced. Include in the background information a summary as to what was found in Part 1. Define the regions via a plot and state the definition of the tropics early (current version of text should have it on line 21 of p. 20369).

9. Section 4.1, second paragraph. I found this analysis of the mean convective outflow height to be interesting. There has been work done (Mullendore et al. JGR 2009) that has determined outflow heights of a storm in South America using radar reflectivity. It would be interesting to compare that one case study to your South America results. Obviously it cannot give validation to model results, but can indicate what might be expected for that region. Plus, it is a good step for bringing global-scale climate studies closer to mesoscale meteorology work.

10. P. 20373, lines 14-26. There is much reporting of how one model compares to another, with little explanation of why that difference occurred. Remember, many readers

C10012

will not know the details of these models and cannot interpret the differences meaningfully. Please explain why differences occur.

11. P. 20373, lines 28-29. The CATT-BRAMS model has the T6h mixing ratios almost completely decaying. Where does it go?

12. P. 20374, lines 23-25. One of the conclusions that I drew from this study is that the PBL parameterization is also important for convective transport of tracers. First of all, the mesoscale meteorology community already knows this for cloud-scale studies. It is generally because each PBL scheme predicts different PBL heights and different stability of the boundary layer which feeds to the convection parameterization. (a) However, I am not familiar with the Louis (1979) PBL scheme. Could the text explain how the scheme works and hopefully include what it tends to do to PBL heights. (b) In Figure 3, the TOMCAT-Louis curves appear to not have any effect from convection. Could the authors examine whether convective transport was occurring or not? I did not see these results in Part 1 of the paper – I wonder how it compares with cloud top distributions, convective intensities, etc.

13. P. 20375, lines 5-9 describe differences between simulations with and without nudging. How good is the analysis that drives the nudging for the Maritime Continent? In other words, is there a paucity of data in that region that can then affect model results? These lines are another place where there is no explanation as to why the results are different. The text only says that the curves are different.

14. Paragraph spanning pages 20375 to 20376. Do the differences between models worry the authors? What do these differences say about the community's capability in predicting locations of convective transport? Answering this question would be a worthy conclusion of the paper.

15. Would MOPITT data or other satellite data be helpful for interpreting Figures 4 and 5? One might expect that biomass burning would affect the MOPITT CO at 200 hPa,

C10013

but at 90 hPa it may have a minor signal and CO enhancements may be found. Just a thought.

16. P. 20376, lines 25-27 seem to be too detailed. What is the reader suppose to learn?

17. P. 20376, line 28 to P. 20377 line 6 compare the T20 seasonal cycle to the precipitation rates in Part 1. In my opinion, this discussion should come sooner in the paper. Second, why should T20 (20 day lifetime) be correlated with precipitation rates? I would think that T6h would be better correlated.

To me, the model results are showing that PBL tracers are lofted to the 150-200 hPa region (Figure 1) and then lifted by large-scale air flow to <100 hPa which explains why T20 is much more abundant above the 100 hPa level. Is this the conceptual picture the authors are trying to convey?

18. P. 20377, line 16. Again, why use T20 to locate rapid transport by convection. It seems that T6 would be a better tracer.

19. In Figure 8, only some of the models are plotted. How do the regional-scale models compare with the measurements?

20. P. 20379, lines 25-28. Why do models over-estimate measurements?

21. P. 20380, line 20. I thought the step-and-stare mode of TES gave better vertical resolution compared to the Global Survey mode. Was not the step-and-stare mode used?

22. P. 20381, lines 6-17. Why do the models underestimate TES data?

23. Discussion section: The results of the TOMCAT-Louis simulation are brought up again as an important result, however there is no explanation of what causes the result.

C10014

Please include discussion of the characteristics of the Louis scheme that are causing differences in model results.

24. Discussion section: The implication of using the same ECMWF data to drive the models yet getting different results in convective outflow height is that the ECMWF data is not controlling the outcome. Instead, the internal model dynamics and physics are critical to the tracer transport profiles.

25. P. 20383, lines 5-15. The authors state that the timing of convection is not important for average mixing ratios in the UTLS. I would agree with that from the perspective of what is entering the stratosphere. However, timing is very important for the PBL stability (as is said in the next sentence) and for emission strength of PBL tracers which then affect the magnitude of the tracer lofted to the UT.

26. Lastly, there has been similar work done at the mid-latitudes (and tropics). While these previous studies do not conduct the same type of analysis as this paper, or use the same perspective, it might be smart to cite them as important previous studies that have shaped our current perspective. Ones that I suggest are: Barret et al., ACP, 2010; Barth et al., ACP, 2007; Jacob and Prather, Tellus B, 1990; Lawrence and Rasch, JAS, 2005; Mahowald et al., JGR, 1995; Tost et al., ACP, 2010.

Technical Details.

1. P. 20365, line 15: need just one "of" before Holtslag
2. P. 20368, line 14: should be "similarity"
3. P. 20369, line 5: should be "data were"
4. P. 20374, line 21: remove "for example"; change "compared" to "comparison"
5. P. 20376, line 10: should be "data were"

C10015

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 20355, 2010.

C10016