Atmos. Chem. Phys. Discuss., 10, C9091–C9097, 2010
www.atmos-chem-phys-discuss.net/10/C9091/2010/

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 26 October 2010

This paper deals with the representation of the transport of tracers by deep convection in atmospheric models. The main purpose is to intercompare the set of models involved in the SCOUT-O3 project ranging from mesoscale meteo-chemistry models to global scale general circulation and chemistry transport models. The exercise is based on synthetic tracers of different lifetime and on a CO-like tracer. The CO observations from the SCOUT-O3 campaign are finally used for an evaluation of the models. It is important to intercompare and evaluate models and the results of the present study would be of interest for the community of users of the models involved. Nevertheless, the paper presents important weaknesses and should be subject to major revisions following the comments given below.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

General comments:

I) The title of the paper is misleading and do not correspond to the results when stating "tropical deep convection and its IMPACT on composition…". First, what "composition"? The statement is really vague! Second, the paper is not dealing with the impact of deep convection on the composition of the troposphere, TTL or UTLS. There are indeed no numbers nor figures concerning the impact of deep convection on the budget/distribution of any gazes such as CO, HNO3, O3 etc. In order to do so, the study should have compared results of the models with and without convection, taking into account the problem of large-scale vertical transport as mentioned in Lawrence and Salzmann (2008). Furthermore, the authors should add references to Doherty et al. (2005) and Lawrence et al. (2003) who are pioneer studies concerning this topic and surprisingly not referred to. In order to fit with the content of the paper, the title should be more something like "Intercomparison of the representation of tropical deep convection in atmospheric models – Part 2: Tracer transport".

II) This paper is presented as the second of 2 twin papers dealing with the representation of convective transport in models. The first paper where modeled meteorological parameters are presented, Russo et al. (2010), is often referred to and presented as essential to the understanding of the present one, but it is "in preparation"! We could have expected that "Part 2" comes after "Part 1" or at least at the same time. With the discussion forum on ACPD, ACP provides the best way to have simultaneous access to complementary papers. I think that the authors should have submitted the two papers jointly to ACP. The reviewer's work is partly hindered by the impossibility to access R2010. I therefore do not recommend publication of the present paper until R2010 is published and accessible (on ACPD for instance) to the readers.

III) The paper is based on simulations from 14 different models/versions of models but the number of models represented in the different plots varies from 11 in Fig. 3 to 5 in Fig. 8 without explanations. Why are there only 5 models in section 4.4 and Fig. 8? The comparisons between simulated CO and SCOUT-O3 measurements is a very

ACPD
10, C9091–C9097, 2010

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

interesting part of the paper but is weakened by the absence of 9 models among 14. Results from the 14 models should be discussed in each section and displayed on each figure (except global maps, tropical means or long time series for mesoscale models). The absence of some models from some part of the paper and/or some figures should be an exception and thorough fully explained and justified.

Specific comments:

Introduction

p20359 lines 10-23: the authors should refer to Tost et al. (2010) who have studied the impact of different parameterizations (in the same model) of deep convection on atmospheric chemistry.

4.1

There are a lot of references to R2010 which is not aceesible! P20369 lines 18-19: the boundaries of the selected regions are very important but it is detailed in R2010!

P20370 line 13-15: the authors state "the meteorological analysis of convective properties in R2010 will help to attribute differences in the model's convective transport". Without access to R2010 I cannot fully understand the differences in the "model's convective transport" mentioned. Are there plots of entrainment/detrainment, and vertical mass fluxes in R2010?

P20370: the convective transport of pTOMCAT−tropical has already been compared to other models over West Africa (Barret et al., 2010) with results in agreement with what is shown here. This should be mentioned when discussing the altitude of outflow of pTOMCAT−tropical relative to the other versions of pTOMCAT.

P20372 lines 10-15 and 21-22: the discrepancy between UMUKCA−UCAM−nud and the other models over WA is very serious and the explanation is not completely convincing "this is thought to be due…". Why is the representation of surface properties deficient in this particular model over this particular region? The cause of the problem

ACPD

10, C9091–C9097, 2010

should be addressed with comparisons of the fields incriminated.

P20374 lines 26-27: I didn't fully understand the explanation about the turbulent mixing. Is it the height of the boundary layer which is too low with the Louis scheme with convection entrainment higher than the top of the Louis boundary layer?

4.2

The authors have chosen 3 different convective regions/season to compare the models. They should mention the Indian/Asian summer monsoon which has been shown to be responsible for the uplift of large amounts of gazes emitted at the surface to the lower stratosphere where they are trapped within the Tibetan anticyclone (see e.g., Park et al., 2007 and Randel et al., 2010).

p20375 lines 20-25: the text discusses fig. 4 which shows mean annual enhancements at 90 hPa, while Ricaud et al. (2007) suggest an important convective enhancement in the UTLS only in March-April-May and not over a whole year.

P20377 lines 11-12: the present study shows that above WA, convective transport reaches 200 hPa but not 90 hPa. The results presented here agree with results from the AMMA project (dedicated to the study of the WA monsoon). Studies based on spaceborne (Barret et al., 2008) or airborne (Law et al., 2010, Fierli et al., 2010) measurements have already shown that convective outflow over WA during the monsoon is maximum at around 200 hPa and is not affecting altitudes above 150 hPa.

4.3

High correlations between surface CO mixing ratios in convective areas and UTLS CO mixing ratios seems rather natural. It is also quite normal that the models show such a correlation, especially when the convective areas have been selected based on results from the models themselves. Furthermore, the difference between models with high correlations (FRSGC/UCI, Oslo CTM2 and UMUKA−UCAM−nud) and models with low correlations (TOMCAT and pTOMCAT) just highlight that convection is not reaching

153 hPa with the low correlation models. This result has been discussed previously and illustrated with Fig. 1. Therefore, unless the authors find something more to state about these correlations, I think that section 4.3, Fig. 7 and Table 3 should be removed from the paper.

4.4

This first part of this section is interesting because it confronts model results and observations from the SCOUT-O3 campaign.

P20378 lines 26-28-P20379 lines 1-9: the description of the observations should appear in a dedicated section after the models description. Section 4.4 is dedicated to the description of results.

P20379 line 15: "On the 16th November air in the outflow of a hector was sampled". Why are there such big differences between the different flights on November 16 as can be seen in fig. 8? It seems that one flight was not "convective".

P20379 lines 25-28: "On the 30th of November... lower down, however there are substantial over-estimation of the measured values by all models". Concerning the lowermost layers, this is almost true for the 4 days, with measured values of about 80 ppbv and modeled values mostly higher than 100 ppbv. The models produce a vertical CO sharp gradient which is not observed with the aircraft measurements. Nevertheless, the plots start at 600 hPa which is very high. Why? How do the modeled and aircraft profiles behave lower down? The part below would give an indication of the boundary layer height as seen by the aircraft and of the behavior of the model to represent this layer (this is interesting regarding what has been discussed before). As it is, it seems that the models are mixing polluted air masses above the real boundary layer. Concerning the free troposphere, it is true that the highest discrepancies between models and measurements are displayed on November 30.

P20380 line 10: "a lifetime of around 3 months". Can you give the reference for such

ACPD
10, C9091–C9097, 2010

a long lifetime? The lifetime of CO in the troposphere is usually ranging from 1 to 2 months depending on location, altitude and season.

P20380 lines 14-25: TES CO measurements should be described in the measurement section together with the SCOUT-O3 measurements.

P 20381: this second part of the section is really weak and limited to a crude description of Fig. 9! First, the results are absolutely not discussed and linked to what has been shown and discussed before. Second, satellites and models are producing maps. Why are there no maps of comparison? Third, TES measures about 2 independent pieces of information in the troposphere for CO. Why a comparison of free tropospheric CO is not given? This would help the discussion concerning the behavior of the models in the mid-troposphere.

5 Discussion:

P20382 line 25: "all of the models" is not right. As stated above only 5 out of 14 models are discussed in section 4.4 and displayed in Fig. 8 and 9!

P20382 line 27/P20383 line 4: as mentioned above, I find section 4.3 rather weak and I think the same about the summary given here.

P20383 lines 5-24: the statements are very general about transport modeling and the discussion should be more focused on the particular results provided and on the models evaluated in the present study.

Figures:

Fig 1: the top altitude should be changed to 50 hPa in order to improve the visibility of the many profiles on the plot.

Fig. 3: the log scale on the x-axis makes the differences between models almost unreadable. A linear scale from 0.1 to 1 would make this plot much more useful. The difference at lower mixing ratio/higher is already present at 0.1 ppt and can be

ACPD

10, C9091–C9097, 2010

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

discussed in the text.

In general there are some typing and syntax errors that have to be corrected.

Refs:

Barret et al., ACP, 8, 3231–3246, 2008.

Doherty et al., ACP, 5, 3205–3218, 2005.

Fierli et al., ACPD, 10, 4927–4961,Âă2010.

Lawrence et al. GRL, 30, 1940, 2003.

Law et al., ACPD, 10, 15485–15536, 2010.

Lawrence and Salzmann, ACP, 6037–6050, 2008.

Park et al., JGR, 112, D16309, 2007.

Randel et al., Science, 328, 611–613, 2010.

Tost et al., ACP, 10, 1931–1951, 2010.

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

ACPD

10, C9091–C9097, 2010

Interactive Comment