Atmos. Chem. Phys. Discuss., 10, C3099–C3108, 2010
www.atmos-chem-phys-discuss.net/10/C3099/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of West African Monsoon convective transport and lightning NO_x production upon the upper tropospheric composition: a multi-model study" *by* B. Barret et al.

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Received and published: 25 May 2010

On behalf of all co-authors, I first want to thank the 2 reviewers for their comments and corrections that have helped us to improve the manuscript.

Answer to the comments by M.G. Lawrence (referee 1)

1)My main comment concerns the interpretation of the Conv-off sensitivity simulation, for two reasons.

First, this builds on the LiNOx-off simulation. However, it is shown that LiNOx has *a substantial impact on the results, so that simulations with and without convective transport that either include or exclude LiNOx could be anticipated to yield different results, and simulations with LiNOx would be more closely representative of the rôle that convective transport is playing in the baseline run. The authors should consider including this alternate form of the Conv-off run (and contrast the results to the present simulation, which might be quite interesting for this region).*

We quantify the impact of convective transport using the difference between the LiNOx-off and (Conv+LiNOx)-off simulations. As mentioned by the referee, quantification based on the differences between the Baseline and a Conv-off simulation would yield different results, in particular because of the convective mixing of ozone produced by LiNOx in the UT. Our approach is based on the idea that convection and LiNOx are physically linked and that switching off convection imply switching off LiNOx. This approach is clearly explained in the manuscript and we think that adding new results based on a second approach would damage the clarity of the manuscript.

Second, and more importantly, the "traditional" interpretation of these kinds of simulations has been shown by Lawrence and Salzmann (ACP, 2008) to be inappropriate. The issue is that even when the parameterized convective transport is turned off, there is still a substantial amount of transport occurring in the large scale winds (which drive the model's advection scheme), which in reality is occurring in deep convective cores. The amount of "leftover" transport varies regionally, depending on how much of the convection is connected with large-scale circulations like the Hadley Cell (e.g., in the ITCZ a very large fraction of the convective transport is left over even when the parameterized convective transport is turned off). This does not invalidate the Conv-off simulations done here, it just means they should be interpreted a bit differently with reference to the discussion in Lawrence and Salzmann (2008)...

We agree with the general comment by the referee concerning the interpretation of simulations with convective parameterisations turned off. We therefore have inserted the following text at the end of the introduction of the "Intercomparison of convective

ACPD

10, C3099–C3108, 2010

Interactive Comment

transport within the WAM region” section:

“Nevertheless, as mentioned by Salzmann and Lawrence (2008), in the tropics an important fraction of convective transport is occuring within the ascending branches of the large scale Hadley or Walker cells. Part of the convective mass flux is therefore already accounted for by the large scale winds used to drive the advection schemes of the models. It implies that, even when parameterised convection is switched off, a large part of convective transport is still occuring in the simulations. This is especially true over West and central Africa during the monsoon that corresponds to an ascending branch of the Hadley cell. From our Conv-off simulations we therefore quantify the impact of parameterised convective transport rather than net convective transport.”

In order to be consistent, we have changed formulas such as “impact of convective transport” into “impact of parameterised convective transport” in the text.

...In particular, the strong effects seen in Figure 5 in southern-central Africa are likely to be associated with “local” convection in MCSs, especially given the low-level convergence (upwelling) seen in Fig 1b that does not continue into the UT (compare Fig 1a); on the other hand, the large scale upwelling in the upper troposphere of Fig 1a is mostly associated with deep convection (this can be gleaned from the high lightning flash frequencies seen here in Fig. 10), which will still be represented (artificially) in the Conv-off simulation. This is also likely why a larger signal in the difference in CO (Fig 5) is seen in south-central Africa than over central Africa (though much of this difference of course has to do with the biomass burning distribution). If the authors could delve into this some it would strengthen the overall analysis and make it more consistent with what is going on in the model simulations (and contrast this more precisely with what is really going on in reality).

We took the referee comment concerning the interpretation of Figure 5 and the differences of convective regimes between south-central Africa and central Africa into account in order to improve our interpretation. It concerns the second part of the sec-

ACPD
10, C3099–C3108, 2010

Interactive Comment

tion “Impact on the CO distribution in the WAM upper troposphere” about the analysis of the differences between LiNOx-off and Conv-off simulations. We have added the following text at the end of the section:

“As already mentioned, our approach allows us to quantify the impact of parameterised convective transport rather than real convective transport on the CO distributions. We discuss here the possible artefacts implied by this approach. Convection above central Africa, north of the equator, is associated with the monsoon and the large scale Hadley circulation. On the contrary, above south-central Africa convection which is less important (see Fig.1) is probably linked to local MCs but not to the large scale mean circulation. Consequently, the impact of real convective transport is probably underestimated north of the equator and convection may be responsible for a less pronounced CO latitudinal gradient that what is displayed in Fig.6 and Fig.7.”

2) On several occasions, relatively detailed descriptions are given of what is seen in the figures before going on to the analysis and interpretation; in many of these cases. I would suggest to cut back on this some and only point out the most critical details, leaving the rest for the reader to see, though this is a matter of taste and should be decided by the editor and authors.

We agree with the reviewer’s comment concerning some Figures. We have accordingly shortened or summarized the descriptions of Figure 2, Figure 4, Figure 5/6 (now 6/7), Figures 10 (now 11), Figure 11 (now 12) and Figure 13 (now 14).

3) The focus on the impacts of convection (sorting of the observations in Figure 12) is exclusively on MCSs; why not also include in smaller systems (were they not possible to detect in the tracking)?

The method used to track convective systems is based on satellite observations and 3D lagrangian backtrajectories. The air mass is tagged “convective” when there is a coincidence between backtrajectories and high clouds from MSG (brigtness temperature less than 200K). As high clouds with BT less than 200K does not necessarily

ACPD

10, C3099–C3108, 2010

imply MCS, we have modified the text and replace MCS with “deep convection” or “high clouds”. In particular, the text concerning the aircraft data has been modified as follows:

“The data have been segregated and grouped into a convective (CONV) and non-convective (NOCONV) class, depending on whether or not the sampled air masses have been freshly impacted by deep convection. The split between the two classes has been performed combining 3–4 days backtrajectories and observations of high altitude clouds from the MSG satellite (brightness temperatures less than 200K).”

4) Lat/Lon values on Figure 1 would help a lot with interpreting it while reading the text.

We have the lat/lon values added on Figure 1.

5) P 2259 L 19: “near-zero”; L 20 “Fig 1a”

corrected.

6) P 2260 L 1: the CO and O3 lifetimes vary substantially regionally; can they not be estimated for the region of study from the budget output of any of the models? That would give much more representative values than these rough global values.

We now give tropical values of CO and O3 lifetimes computed with TM4:

“The chemical lifetimes of these two species were computed for the JJA period in the tropical troposphere using the TM4 model. We found 1.2 months for CO and 4 days for O3. This means that O3 is a tracer for local transport such as convective uplift while CO can be transported far away from the active source region within the large scale Walker and Hadley cells.”

7) P 2260 L 17: “...entrainment” in the text, “detrainment” in the caption (I assume the latter is correct, since it fits well with the convective mass flux profiles in most of the figures);...”

We have corrected entrainment for detrainment in the text p2260.

ACPD

10, C3099–C3108, 2010

“...also, the maximum detrainment in panel c (INCA) is not reflected in decreasing convective mass fluxes, which is strange – can the authors explain this (is it perhaps simply a matter of the selected contours)?...”

This is indeed a problem of selected contours. We have added the 0.006 kg/s/m2 contour for the updraft mass fluxes so we can better see that this flux is decreasing (from 0.008 to 0.006) vertically while we reach the maximum detrainment mass flux.

8) P 2260 L23++: There is a recent publication in ACP by Tost et al. (2010, p. 1931-1951) that examines chemistry with different convection schemes and would support this discussion nicely (it builds on Tost et al., 2007, which is discussed later in the manuscript).

We have improved the discussion concerning the differences between models including the findings of Tost et al. (2010):

“The study of Tost el al. (2010) also corroborates our findings. Based on simulations with the ECHAM5/MESSy GCM, they examined the impact of convection parameterisation upon atmospheric chemistry modelling. In particular, comparing global mass fluxes, they show that the KFB scheme is responsible for deeper convective activity than the Tiedtke scheme, with "substantial mass fluxes up to 200 hPa and even higher". They further show that "an almost undiluted transport of CO-rich boundary layer air in the TTL" is responsible for higher concentrations of CO in the UT with the KFB scheme than with the other schemes”

9) P 2263 L 25: “right” is a bit strong, given the many model uncertainties, “appropriate” would be more appropriate here.

We agree and changed “right” to “appropriate”.

10) I think the results that are mentioned with the GFEDv2 simulations are very interesting, and nearly a paragraph is spent on them; I would suggest including the figuresin an electronic supplement for interested readers.

ACPD

10, C3099–C3108, 2010

Interactive Comment

In order to provide the information requested by the referee, we have split Figure 4 into two Figures with results from both the L3JRCv2 and GFEDv2 simulations and corresponding differences. Figure 4 (resp. Figure 5) displays CO (resp.O3) latitudinal transects. The text has been modified accordingly.

11) P 2267 L 10+: the authors set out to "throw new light upon the possible causes for differences among models concerning. . ."; I interpreted this as indicating the differences between the Doherty et al. (2005) and Lawrence et al. (2003) studies, which would have been very interesting, if possible to do, but these are not mentioned again later, just the four models used in this study; the authors should clarify which models are meant in this statement (and if indeed the D05 and L03 studies are meant, then make the connection more explicit in the later text).

We understand that our statement was confusing and possibly misleading. We don't have the pretention to throw new light upon differenecs between L03 and D05. We therefore have clarified the statement :

"..performing simulations with similar emission inventories, our goal is to explain the possible causes for differences among the four models involved in the AMMA project.."

12) Something is mixed up with the last few figures. On P 2272, the callout for Fig. 14 is clearly meaning Fig. 13; then on P 2273 a Fig 13 with tropospheric O3 columns is referred to, but does not exist in the set of figures at the end; on P 2274, Fig. 14 is referred to again, this time correctly; this should be checked and fixed.

The callout on p2272 has been changed to the right figure (now 14 as a new figure has been inserted). Tropospheric O3 columns are indeed present as labelled contours in Figure 14 (previously Figure 13).

13) Grammar corrections

These corrections have been taken into account

Answer to the comments by anonymous referee 2

ACPD

10, C3099–C3108, 2010

Interactive Comment

p2247, line 8 "the baseline : : :" I would recommend to clarify the sentence.

We have shortened the sentence to make it clearer.

p2247, line 15 "with good agreement in the Northern Hemisphere" This statement needs clarification because the figure 4 does not show a good agreement between MOZAIC and models in the Northern Hemisphere expect for INCA (that underestimates ozone in the South Hemisphere).

The referee is right about this sentence and we have changed it into:

"Concerning UT O3, the models exhibit a good agreement with the main observed features. Nevertheless the majority of models simulate low O3 concentrations compared to both MOZAIC and Aura/MLS observations south of the equator, and rather high concentrations in the Northern Hemisphere."

Description of the models: MOCAGE: I would suggest to add the reference of the parameterization used to redistribute Nox emissions on the vertical.

The vertical redistribution of the LiNOx is made by the parameterised convective fluxes as explained in the text and detailed in the cited reference (Mari et al., 2006).

LMDz4-INCA: p2253, line 3: "second-order scheme" -> this point would need to be checked.

We have checked this point and the sentence "...second-order scheme..." has been replaced by:

"The large-scale advection of tracers is based on the finite volume transport scheme of Van Leer (1977) as described in Hourdin and Armengaud (1999)."

p2253, line 9: this point would need to be checked. I would have thought that PR(92) is used for both maritime and continental lightning. In Jourdain and Hauglustaine (2001), Michalon et al. (1999) was only used in a sensitivity study. Please indicate the reference PR (97) for the parameterization of the number of NO per flash (IC and CG) and

ACPD
10, C3099–C3108, 2010

Interactive Comment

mention the parameterization used to redistribute LiNOx emissions on the vertical.

After verifications, the text has been modified according to the reviewer's comments:

"LiNOx are parametrized according to Jourdain et al. (2001) with FF based on PR(92) for both marine and continental thunderstorms. Pickering et al. (1998) is used for vertical redistribution of lightning NOx. The IC/IG ratio is computed according PR(93). According to PR(97)..."

TM4: p2254, line 2: " Marine lightning is prescribed to be ten times : : :." -> I would suggest to add the reference of the work it is based on.

The ref. Schuman and Huntrieser, ACP, (2007) has been added in the text.

Analysis of convective mass fluxes: p2260-2261: I think it would be useful to explain briefly why the detrainement mass fluxes in INCA are very different in term of magnitude and distribution than in the other models.

There was a bug in the way the detrainment fluxes were computed with INCA. It has been corrected (thanks to the reviewer's comment). The differences mostly concerned the amplitude of the fluxes which are in much better agreement with the other models now (see new Figure 2)..

Analysis of the lightning activity during summer over West Africa: P2269-2270: It would be useful to give an explanation for the overestimation of lightning over the Sahel by some models.

A sentence has been added in the paper when discussing the lightning activity:

"This overestimation is a result of the deeper convective activity computed by these 2 models over Sahel than over Central Africa as can be seen in Fig. 2."

Figure 2: The colorbar does not appear correctly.

It now appears correctly.

ACPD

10, C3099–C3108, 2010

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Other corrections:

The colorbar of Figure 6 was erroneous. We have therefore changed the color tables of Figure 6 and 7 (former 5 and 6) in order to have the same colorscales for both figures.

In the initial manuscript Figure 2 corresponded to a zonal average over 10-40°E while it was mentioned 0-30°E in the text. The new Figure 2 corresponds to 0-30°E. There are no significant changes between the two plots.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2245, 2010.