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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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This study examines the factors influencing CO, NOx and O3 over Africa and the eastern Atlantic during the west African monsoon. It makes use of observations from the AMMA campaign, plus MOZAIC commercial aircraft observations and satellite retrievals for this region, along with base and sensitivity simulations from four models. This is a very solid study, with a clear and sensible scientific approach and a well-organized and well-written manuscript, and can be recommended for publication with only a few revisions, mostly minor.

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 role 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).

- 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). 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).

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.

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)?

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

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

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.

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); 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)?

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).

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

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 figures in an electronic supplement for interested readers.

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).

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.

13) Grammar corrections (these are mostly from section 5.2 onwards, so that part of the paper should be re-checked carefully by the authors before final publication; only the corrected forms are written below):

- P 2256, L 18: something is odd here

- P 2263, L 27: “detrainment. . .occurs”

- P 2264, L 24: “dependence”

- P 2264, L 28: “These altitude differences”

- P 2265, L 13: “MOCAGE overestimates”

- P 2265, L 24: “The differences. . .are. . .”

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- P 2266, L 9: “in detail”
- P 2266, L 9-10: “simulations”
- P 2267, L 19: “These O3 enhancements”
- P 2268, L 4: “lesser extent, which both. . .”
- P 2268, L 20 and 22: “CTMs”
- P 2275, L 15: “occurs”
- P 2276, L 14: “Atlantic”
- P 2276, L 18: “the Sahel”
- P 2276, L 18-19: “none of the models are able to. . .”

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

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