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Interactive comment on "Cloud-venting induced downward mixing of the Central African biomass burning plume during the West Africa summer monsoon" *by* Alima Dajuma et al.

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Referee #1 We thank the Reviewer for her/his time to review our manuscript and for the constructive criticism. Please find below our point-by-point answers in red, modifications of the manuscript are in blue while the original Reviewer's comments are in black.

General Comments The manuscript has an important goal–investigate the hypothesis that "cloud venting plays a considerable role in the downward mixing of biomass burning aerosol" (line 74). Achieving that goal would be of great interest to everyone in the field because so little is known about it. Unfortunately, the manuscript does a very poor job of investigating the topic. Many figures are shown and discussed, but none directly address the cloud venting issue. There is no systematic laying out of evidence to support the hypothesis. Nonetheless, the authors make unsupported speculations about how cloud venting could be a factor in what is being shown. Sad to say, but after finishing the manuscript I felt that I had learned nothing about cloud venting.

We agree that this aspect was not as clear as it should have been in the first version of this paper. We have now substantially rewritten the section on the tracer experiment to much better support the argument. Tracer experiments with and without turbulent diffusion help to clearly isolate the cloud effect. We hope that this way the evidence is clear and convincing.

Major Issues

1. The Introduction contains no information about previous studies of downward transport by convection (your topic). If any exist, they should be described. Your work should pick up where the previous studies left off.

We agree that this aspect was in fact underrepresented in the Introduction. The following paragraph on downward transport is now included from line 101 onwards:

In contrast, rather few studies investigated the downward transport of elevated pollution through convective clouds. For the marine PBL, aerosol particles from the free troposphere have been identified to serve as cloud condensation nuclei in stratiform clouds with cloud entrainment contributing up to 20% of the aerosol mass (Raes, 1995; Katoshevski et al. 1999). Over land, most studies concentrated on the Amazon rainforest. Based on campaign data during the wet season, Betts et al. (2002) showed that convective downdrafts rapidly transport air with high ozone down to the surface from around 800 hPa, suggesting a significant role of this process for the photochemistry of the PBL and surface ozone deposition. Gerken et al. (2016) even found evidence for transport of ozone-rich air from the mid-troposphere to the surface, enhancing the volume mixing ratio in the boundary layer by as much as 25 ppbv on the regional scale, while Wang et al. (2016) demonstrated the injection of high concentrations of small aerosol particles into the PBL by strong convective downdrafts. In more general terms, Jonker et al. (2008) proposed a refined view of mass transport by cumulus convection relevant for the dispersion of aerosol. According to them, the descending motion near the cloud environment is significant and rather different than in a distant cloud environment, which is characterized by more uniform and quiescent dry descending motion.

2. Line 150–It is unclear how the case study of 02-03 July was conducted. Did you use the COSMO simulated meteorology, but instead of the MOZART chemistry, you specified the simplified CO profile? The details of the case study methodology must be better described. ***This description will be critical to the success of the manuscript. Readers must know what you did before they can assess what the results show.

We acknowledge that this description was not as detailed as it should have been. The following information has now been added to the modelling section 2.2 in line 195:

Both domains, D1 and D2, were run with the parametrization for deep convection switched off and using the two-moment microphysics scheme (Seifert and Beheng, 2006). Over D1, the modelled period ranges from 25 June–31 July 2016 with the meteorological state being re-initialized every day at 00 UTC. ICON operational forecasts at 13 km grid spacing with 90 vertical levels are used as meteorological initial and boundary conditions and MOZART chemistry with a grid mesh of 280 km x 213 km and 56 vertical levels for the pollutant initial and boundary data. Cloud condensation nuclei are prescribed with a constant aerosol number concentration of 1700 cm-3. The purpose of the D1 simulation is to compare the model output and observations for monthly mean conditions, i.e., for July 2016, after a six-day spin-up.

3. State the reason why you chose 02-03 July for the case study. Was it typical of all July days or was it picked for a particular reason?

Good point. We added the following text to section 2:

The period 02–03 July 2016 was chosen because it falls into the post onset phase of the monsoon, characterizing an undisturbed monsoon condition, and is thus favorable for process studies (Knippertz et al., 2017; Deetz et al., 2018).

4. Line 221–Observed vs. simulated CO in Fig. 4. There are some major differences in the two fields. You say that features are "reasonably captured". That is a very generous assessment that I believe should be 'toned down'.

This sentence was modified according to your comment (line 264):

Overall the spatial patterns of CO concentration are captured by the model with some discrepancies.

5. Line 240-1–As mentioned in the General Comments, I maintain that you do not 'especially focus on the role of convective clouds'. That is my fundamental problem with the manuscript; the results seldom address the hypothesis.

We have now created a new section 5 named “Downward cloud venting” that is fully dedicated to this topic. We have run additional tracer experiments with suppressed turbulent diffusion in case of the tracer to better isolate the effects of downward cloud venting. Turbulent diffusion is accounted for for all other variables (wind, temperature, humidity, hydrometeors). We also created a new figure in the summary section to better illustrate the mechanisms at play (see Fig. 12 in the manuscript). Here is what we wrote in this new section at line 424 in the manuscript:

In general, processes that can support the transport of biomass burning aerosols from free-tropospheric layers into the PBL include: (i) large-scale subsidence and thus vertical advection (Katoshevski et al., 1999), (ii) turbulent mixing into the marine PBL, and (iii) vertical transport by convective clouds. With respect to point (i) we can state that the cross sections in Fig. 8 do not show clear indications of a systematic sinking of the biomass burning plume suggesting that for the situation presented in section 4 synoptic-scale subsidence is not a leading factor. To investigate the relative importance of processes (ii) and (iii), we designed an idealized tracer experiment. For the simulations starting at 02 July 2016 at 00 UTC initial profiles of a tracer were prescribed within the domains D1 and D2. The idealized tracer has a concentration of 1 ppmv between 2 and 4 km and is zero elsewhere. Chemical reactions as well as dry deposition are neglected in order to isolate effects of transport only. At the lateral boundaries the tracer concentrations were held constant at the initial profile such that only mixing within the domain can change tracer concentrations. Two sets of simulation were done: one with and one without turbulent diffusion. The idea behind this is to separate this effect from that of downward cloud venting. The simulations were carried out for a period of two days (2–3 July 2016).

6. Line 275–"conspicuous north-south orientation" of cells. I don't see this.

This statement has now been updated at line 320 in the manuscript.

The largest and most intense convective systems are simulated over the ocean with a pronounced north–south elongation along the southwesterly monsoon flow. These were persistent throughout the day (not shown).

7. Section 4.2–The oceanic area should be the focus of this section. While it is good to have simulated/observed agreement over land, it is especially important that this area be well simulated because you later will show cross sections and area averages in this region. So . . . a change in focus is needed in this section. We modified this section slightly to emphasis the ocean a little bit. However, in the discussion of Fig. 9 we also have a part on the land situation and therefore think that the description we provide here is necessary.

8. Figure 6– Panel c) contains simulated precipitation rate at 1800 UTC while panels a) and b) are for 1200 UTC. Why the time change? The text says nothing about convective

C2 evolution between the two times. Also, does panel b) comprise the same area as the other panels? There are no lat/long indications on b).

The reason for this choice is to illustrate the appearance of convective cells prior to the rainfall. Concerning, panel b) it corresponds to the same area and indications of latitude and longitude have now been added. The figure below presents the evolution of the convective cells between 13 UTC and 18 UTC. They appeared over the Gulf of Guinea around 7 UTC from were persistent throughout the day.

Temporal evolution of convective cells from SEVIRI cloud visible image EUMETSAT from 13 UTC to 18 UTC (from http://nascube.univ-lille1.fr).

9. Figure 7 does not cover the same domain as Figs. 5 or 6. This makes a comparison of features very difficult. I suggest you place a dashed line at 4 N as done in earlier figures. Please outline the area in Fig. 7 that corresponds to the area in Fig. 6. Also, Fig. 8 will show that the level of maximum CO is 2000 m. That would seem a better choice for Fig. 7 than 2900 m. Or :you could add 2000 m as a third panel to Fig. 7. We agree with your suggestions. Fig. 7 has now been replaced by the figure below. The text was modified in the following way (line 351):

Concentrations over the nested domain D2 at 500m (Fig 7c) are moderated with traces of higher CO concentrations over the Gulf of Guinea, some smaller elongated plumes (e.g., from Abidjan and Accra), and much elevated levels downstream of Lake Volta. As concentrations above ground level are shown in Fig. 7c, the elevated values over the Atakora chain at the border of Ghana with Togo are at least party related to the fact that higher ground is closer in the vertical to the main midlevel pollution plume from Central Africa.

10. Line 311–There is no justification for this transport statement.

Areas where concentrations are low at 500 m and elevated at 2000 m cannot be dominated by local sources at the surface. Based on previous studies, the bulk of biomass burning plume is located between 2 and 4 km of altitude. Furthermore, our simulations with and without biomass burning (not shown) confirm that high concentration found at 2000 m results from biomass burning emissions. We there argue that this statement is justified and did not change it.

11. Line 313–I do not see a CO feature at 2900 m that is "west and north of the main plume". Also, what is your rationale for stating that this could be due to downward mixing?

We agree that this is not easy to understand. We changed the text in the following way:

The elevated concentrations at 500m over the ocean to the west and north of the main plume at 2000m suggest downward mixing into the PBL from aloft.

12. Line 331–How does Fig. 8 indicate that the biomass plume is advected in a westerly direction?

You are right that this is an overinterpretation. We changed the sentence in the following way:

There is a clear band of high CO concentrations of up to 400 ppbv, mostly between 1 and 3.5km over D2, which is the signature of the long-range transport of the biomass burning plume from Central Africa (Mari et al., 2008; Zuidema et al., 2016), possibly affected by larger-scale subsidence.

13. Line 341 and Fig. 8d (cloud liquid water)âĂŤHow does this panel indicate positive and negative vertical motions?

We agree that this was misleading. The respective section now reads:

Figures 8c and d show meridional-vertical cross-sections of, respectively, CO concentration and specific cloud liquid water content along 6°W, close to where convective activity is seen in Fig. 8a. Areas of high cloud liquid water are collocated with minima in CO, supporting the idea of cloud-induced transport and mixing. The most promi-

nent of such areas is located around 4.3°N, where significant amounts of cloud water stretch from below 500m to almost the top of the biomass burning plume, leading to an substantial erosion.

14. Line 345–What do you mean by "less evidence of convective mixing"?

What we mean here is that, Fig. 8e does not show the sharp gaps in the pollution plume as evident from Figs. 8 a and b. This is now explicitly mentioned in the text:

At 4°W (Fig. 8e) there are no pronounced gaps in the pollution plume, suggesting less convective mixing at this time than at 6°W but concentrations at low levels are not much different.

15. Line 361, simulations initializedâĂŤIs this truly a new and different model run initialized on 2 July? I had assumed that you were using the meteorology for 2-3 July from the simulation begun on 25 June (the run you have been describing up to this point). However, now you have used the simple CO profile, not the MOZART-derived CO. I am confused. You must explain what you are doing. This is the same issue raised in question 2 above.

No, these are in fact new simulations as now clearly explained in the text.

16. Figure 9–You show the layers below 1 km, between 2-4 km, and the sum "between the two". Why did you not include the layer between 1-2 km?

We agree that this should be included. The mass calculation between 1-2 km have been now added in Figures 9 and 11 corresponding to the SWA and regional domains, respectively.

17. Line 399 + and Fig. 10–I do not understand the purpose of relating SST to % mass. How does this relate to cloud venting? You must explain the relevance of this figure to your hypothesis.

This Figure (Fig. 10) investigates the influence of the SST on the transported mass of the tracer to below 1km after two days of integration with respect to latitude. This is related to the cloud venting, because in the discussion of Fig. 9 we show that this is the dominant process in the vertical transport. Here we see that SST modulates the mass transported into the PBL. Based on the findings from Fig. 9 we assume that this due to clouds. We rewrote this entire section to explain that better.

Minor Issues 1. Describing locations by city names should be avoided. Most readers will have insufficient geographic knowledge of these city locations. Either provide a map showing all cities that are mentioned or completely avoid the use of city names.

Fig. 1 has been replaced by figure with city names.

2. Line 146–What are the horizontal and vertical grid spacings of the ICON and MOZART data that are used for your ICs and BCs? State these in the text.

This has now been added to the text at line 198 as follows:

ICON operational forecasts at 13 km grid spacing with 90 vertical levels are used as meteorological initial and boundary conditions and MOZART chemistry with a grid mesh of 280 km x 213 km and 56 vertical levels for the pollutant initial and boundary data

3. Line 151–Is your model configured with one-way or two-way interactions between D1 and D2? State this in the text. This has now been updated.

4. Line 165–I assume that D1 and D2 are being run concurrently. However, line 165 ("over D1") gives me doubt. Please re-phrase to make this clear.

D1 was run first and then D2 is nested into the coarse domain D1. This was re-phrased as follow in the manuscript at line 204:

we analyze a particular case study on 02–03 July 2016 simulated over D2 using the outputs of D1 for both meteorological and chemical initial and boundary conditions. The TMMS was combined with the prognostic aerosol, this way accounting for aerosol

direct and indirect interactions.

5. Figure 2 should be in color like the other figures.

This figure has now been updated.

6. Figure 3b–The abrupt gradient in MODIS-derived cloud cover at 4.5 N is very suspicious looking. Could this be a data problem, or is it real?

The data have been checked and appear to be correct. One possible explanation we mention in the text is areas of coastal upwelling that may locally modify cloud cover.

7. Figure 7 caption–Do you mean that all the cooler regions west of 3 W over the water are cold pools related to convective cells? If only certain regions represent cold pools, those should be denoted by arrows. Also, it would strengthen your argument that these are convectively related cold pools by referring to Fig. 6c which shows that there was plenty of simulated rainfall in the area (at least 6h later at 1800 UTC).

Figure 7 has been updated now and the cool pools are more visible over the D2 domain. The following text at line 368 in the manuscript has been added:

Zooming in on domain D2 (Fig. 7d), concentrations at 2000m are generally much higher than at 500m (Fig. 7c), in particular over the coastal zone. Strikingly some marked “holes” are evident that correspond to areas of cold pools associated with convective cells (see Figs. 5 and 6c), suggesting that in these areas clouds support downward mixing.

8. Line 347 (Fig. 8f)–The panel label for f) says 1 deg W, not 1 deg E.

The caption of Fig. 8f has been corrected. It is 1°E.

9. Figure 9–Dates on the x-axis should be labeled.

This has been added now.

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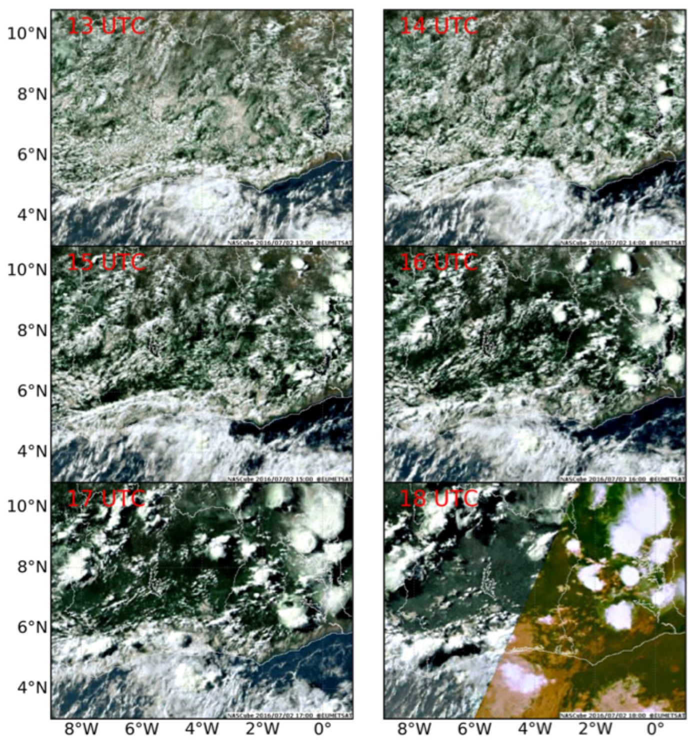
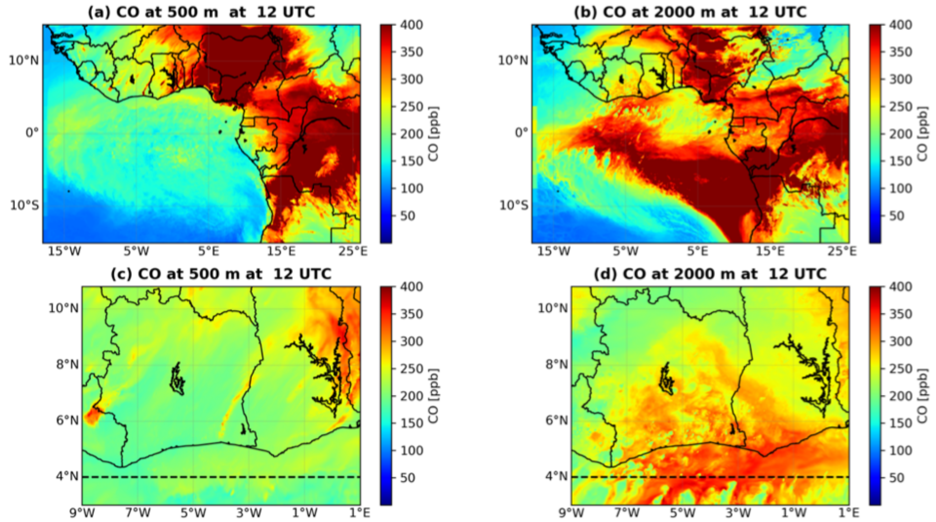


Fig. 1.



(a) CO at 500 m at 12 UTC
(b) CO at 2000 m at 12 UTC
(c) CO at 500 m at 12 UTC
(d) CO at 2000 m at 12 UTC
CO [ppb]

Fig. 2.