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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement and depletion of lower/middle tropospheric ozone in Senegal during pre-monsoon and monsoon periods of summer 2008: observations and model results" *by* G. S. Jenkins et al.

Anonymous Referee #1

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Review of "Enhancement and depletion of lower/middle tropospheric ozone in Senegal during pre-monsoon and monsoon periods of summer 2008: observations and model results" by Jenkins et al. The authors present ozonesonde measurements carried out in Dakar, Senegal during the summer 2008 in order to investigate the ozone vertical distribution during the pre monsoon and monsoon periods. They show how ozone is impacted by Saharan air masses. They use the regional model WRF-Chem to support their hypotheses regarding the variability of ozone between the pre monsoon and monsoon periods. The observational results presented in the paper bring new insight on

the ozone vertical distribution in West Africa. This region was poorly documented before the international program AMMA (African Monsoon Multidisplinary Analysis) and this program has helped the scientific community to gain knowledge in atmospheric chemistry during the West African monsoon season. However a number of questions remain, especially regarding the interactions between aerosols and gas phase chemistry. The objectives of this study, were they reached, would be of great interest to the scientific community. The authors suggest that heterogeneous chemistry as well as ozone dry deposition and biogenic NOx from wetted soils would be the mechanisms controlling the ozone distribution in this region. However they do not present any relevant modelling results to support their hypotheses. Their use of WRF-Chem, a state-of-the-art regional atmospheric model, is disappointing as well as the conclusions they take from the model results. As a consequence, I do not think the manuscript could be published in its present form because the description of the criteria to sort the data set is not clear enough to the reader and the manuscript lacks of significant modelling results to support their findings. I would recommend its publication in ACP when the authors have considered a large number of suggestions, which are aimed at clarifying the study and bringing robust results through an appropriate use of WRF-Chem and backward trajectories.

General comments:

Introduction

- Last paragraph of page 28065: This paragraph is confusing because not well organised. Please clarify by specifying the sources of NOx (soils and lightning – and anthropogenic), and then explain the role of transport on ozone and its precursors. The last sentence is very confusing as it mixes dry and wet deposition and as a consequence different compounds. Dry deposition of ozone could be an important sink of ozone in the boundary layer. However I assume the authors refer to the wet deposition of HNO3 in "Wet/dry deposition . . ." Please rephrase this paragraph and sort the different reasons for reduced or enhanced ozone.

- Lines 2-7 of page 28067: This paragraph is important for the understanding of the study however it lacks of clear description regarding the latitudinal bands involved. I suggest the authors add a map with the location of the measurements sites (Dakar and MBour) along with the main features (major air flow, vegetation cover during the period of the study, SAL latitudinal position, Sahelian zone, ...)

Section 2

- Lines 18-19 of page 28067: The authors introduce a sort of the ozone profiles (SA and non-SAL) but there is no explanation of the criteria used to distinguished the profiles. The explanation is given later in the text (end of section 3.1). There might be no need to say at this stage of the text that the profiles have been sorted.

- Lines 1-10 of page 28068: The authors state that they use WRF-Chem for comparison with the measurements, which has not been shown in the present manuscript. As mentioned previously the use of WRF-Chem in this study is disappointing. Some model/observation comparisons and sensitivity studies to NOx emissions would have help the authors to support (or not) their hypotheses. What about the NOx emissions from soils, from lightning in WRF-Chem? How are the vegetation cover and the dry deposition treated in WRF-Chem?

- Lines 12-13 of page 28068: There is no explanation for the onset criteria. If linked to the occurrence of rainfall in Dakar, then use Fig 2d separately to assess the onset date.

Section 3

Section 3.1: This section is not well organized and confusing to the reader; I would suggest the following changes:

-First, clarify the onset criteria based on Fig 2d (as a separate figure)

-Second, clearly define the SAL criteria used to sort the profiles (based on the RH measured by the sondes). Also adding this information (SAL/non-SAL) in Table 1 would

be helpful to the reader. Then discuss Fig.3a.

-Third, discuss TCO and AI/AOT and clarify the usefulness of these data sets to the study (agreement) Section 3.2

-Lines 26 of page 28070: The author state that the OMI values are >2 on 8, 10 and 15 June, which is not true for June 10 based on Table 1. As the criteria for SAL profiles was not clearly defined, the reader does not understand if June 26 is a SAL event or not. . . as the aerosol loading is greater than on June 10 (Tab.1).

-Lines 12-18 of page 28070: The wind profiles are not shown in the manuscript. Do they agree with the streamlines shown? Have the author tried to run backward trajectories to support the air masse origin and the load of aerosols? Running backward trajectories could help to assess the history of the air masses and the production/destruction/import of ozone within the air masses.

-Fig5: This figure is too small; also it is hard to distinguish the continent from the streamlines. Adding a dot for Dakar would be helpful to the reader; Please make sure this figure is larger and clearer for the next version of the manuscript.

-Lines 19-21 of page 28070: This last sentence is speculation, as the authors do not show any evidence (modelling results or other measurements) of ozone reduction through heterogeneous chemistry. Reduced photolysis rate cannot be ruled out here.

Section 3.3

-The first sentence should be used in Section3.1 when the onset is defined.

- Lines 3-7 of page 28071. Between June 26 and July 2nd, we observe a difference of 10 to 20 ppb between 950hPa and 650 hPa. Such a deep ozone increase is likely not caused by enhanced ozone production due to NOx-biogenic emissions only. See Saunois et al., 2009 for insight on the vertical extent of biogenic emissions impact, which is limited to the lowest level of the troposphere. Again backward trajectories would help to determine the origin of the air masses sampled on June 26 and July 2nd and how the sampled air masses were differently impacted by the environment. The author should also consider the impact of lightning NOx on ozone production, as this NOx source will likely affect a deeper part of the troposphere. Do the RH and wind profiles help clarifying the differences of the sampled air masse? Here an appropriate use of WRF-Chem (sensitivity studies on NOx source, assessment of ozone tendencies -chemistry, convection, transport- using the model diagnostics) is needed to clarify the differences observed between the two profiles. Combining backward trajectories and an appropriate use of WRF-Chem is necessary if the authors want to discuss the measurements – as stated in their objectives. Section 3.4

-Lines 9-12 of page 2807: This sentence is confusing: instead of increase/decrease use higher/lower concentrations, unless the authors mean decrease/increase with altitude; If so, then specify the pressure level range.

-The paragraph needs to be reorganized because the information is mixed: for example based on RH profiles, first discuss the SAL event of August 2. Then discuss the three others and their differences. Are these differences due to convective transport?

-Fig8: This figure is too small; also it is hard to distinguish the continent from the streamlines. Adding a dot for Dakar would be helpful to the reader; Please make sure this figure is larger and clearer for the next version of the manuscript.

Section3.5

As said previously this section is disappointing. A specific section should be devoted to the modelling part. The suggested work (backward trajectories, sensitivity studies to NOx sources) should be carried out in order to seriously discussed and support the mentioned hypotheses that could explain the differences observed in the ozone vertical profiles. Also in such modelling studies, model versus observation comparisons are necessary to assess the capability of the model and the factors playing a major role to reproduce the observed ozone profiles.

- Lines 12-16 of page 28072: The authors give three reasons for elevated O3 concentrations on June 12. The first one, (a) stratospheric intrusion, is the only one tested here. However the results from the simulation are not convincing. Why different initial dates are needed to test this hypothesis? How the model compares with the observed ozone profiles? Why showing and discussing ozone concentrations up to 100hPa – Line 1-5 of page 28073-? Moreover the high RH measured on June 12 (Fig4b, RH = 80% up to 500hPa) tends to reject such stratospheric intrusion event. As a consequence this modelling study is not convincing to explain the high ozone concentration observed on June 12. Why the authors have not tested the other hypotheses?

- Lines 14-19 of page 28073: The comparison between the three profiles should have been done earlier to introduce the modelling study and its objectives.

- Lines 21-25 of page 28073: The authors suggest different factors that could explain the differences observed in the ozone profiles. These different hypotheses should be addressed using sensitivity studies with WRF-Chem, along with backward trajectories. It appears here that convection and its consequences (lightning-NOx emissions, vertical transport of precursors (biogenic NOx, e.g.) to the UT and subsequent O3 production) play a role. This should be investigated.

- Line 5 of page 28074: here again " dry and wet deposition" is confusing. Please clarify

- Line 5 of page 28074: The hypothesis of ozone poor air masses could be tested using backward trajectories (along with ozone distribution from satellite, e.g.).

- Lines 9-14 of page 28074: There are two major comments in this sentence. First the study Saunois et al., 2008 deals with ozone in the UPPER troposphere (and not in the lower troposphere as stated by the authors) and was based only on MOZAIC data (and not on AMMA measurements as stated by the authors). A gradient is observed in the upper troposphere with a minimum at the ITCZ due to vertical transport of ozone poor air masses from the surface to the upper troposphere by convection. As a consequence, this study is not relevant for the lower troposphere but might give insight on the influence of convective transport and lighting NOx induced ozone production. Which pressure levels are involved here? Please clarify this point too. In the references, the authors cite Saunois et al., 2009, modelling study in the boundary layer showing that the ozone minimum is controlled by dry deposition. Second the TCO shown in Fig.1 present an ozone minimum collocated with the ITCZ, and with similar northward migration between June and August. The minimum of TCO observed is probably linked to the vertical transport of low ozone from the surface by convection. The sensitivity of the satellite instrument to the lowest part of the troposphere (below 900 hPa) is probably not significant enough to see the effect dry deposition.

- Line 15 of page 28074: the relationship between ozone and water vapour is not clear. Which relationship are the authors talking about?

- Line 15 to the end of page 28074 with Fig 11: How the model results compare with the observations? Please change the x-axis from hours to day of month for clarity. The model should be used to interpret the model results and the observations: what are the evolutions of the NOx, CO, COV concentrations over this time period – as simulated by the model. Maybe the use of satellite data for NO2 and CO will also be helpful here. The only ozone and water vapour distributions are not enough for the discussion.

Section 4

- Lines 2-3 of page 28075: The results stated in this sentence have not been clearly proved.

- Lines 5-12 of page 28075: Such clear description with altitude range would have been helpful at the beginning of the text. . .

- Lines 20-25 of page 28075: The description of the different layers includes statement on loading of aerosols, yet the authors do not present any vertical profile of aerosols. As a consequence, there is no proof (in the paper) of such different loading in aerosols

with altitude.

- Line 27 of page 28075: Saharan soils? Or Sahelian soils?

- Lines 1-10 of page 28076: The suggested mechanism has neither been tested within a model nor supported by measurements of other compounds involved (NOx, HNO3, aerosols). As a result, this mechanism remains at the stage of assumptions (same as in the introduction).

- Lines 19-27 of page 28076: this paragraph should be removed or revised with new appropriate modelling studies as suggested above.

- Lines 1-7 of page 28077: Idem. This assumption has not been tested. See previous comment on the vertical extent of biogenic NOx impact. . .

- Lines 8-10 of page 28077: “dry and wet deposition”: the phrasing is confusing, specify which compound is dry deposited or scavenged by precipitation. . .

Technical & Minor comments:

- Line 3 of page 28064: Please specify “. . . anthropogenic emissions, which act as sources of ozone precursors”. Also the phrasing is confusing. Dry deposition and heterogeneous chemistry are sinks for ozone. However the authors seem to forget that photochemistry is not always a net positive production of ozone and as a consequence could be a sink as well. Please clarify.

- Lines 14-16 of page 28064: the influence of biomass burning transported from the southern hemisphere to the Guinean coast during the boreal summer was first suggested by Sauvage et al., 2005, 2007. In the framework of AMMA a number of studies have followed, the first one being Mari et al., 2008. Please complete the bibliography of this part of the introduction.

- Line 16 of page 28064: “Further north-west” instead of “further north” would be more appropriate.

- Line 28 of page 28064: please specify the pressure levels in the SAL features that would help the reader to understand.

- Line 11 of page 28065, “radicals” instead of “precursors” would be more appropriate.

- Line 13 of page 28066: There have been modelling studies that have estimated the impact of biogenic NOx on ozone levels in the framework of AMMA: Delon et al., 2008 and Saunois et al., 2009. Please add those references. Also the lecture of Saunois et al., 2009 would give insight on the vertical scale of the impact of biogenic NOx on ozone.

- Line 28 of page 28066: the relationships between “low ozone and dry deposition over vegetation” and “high ozone and high biogenic N0x emissions” showed by the measurements and presented in Reeves et al., 2010, have been supported and confirmed by the modelling study made by Saunois et al., 2009. Please add this reference within the introduction.

- Line 22 of page 28068: The use of “dry/wet deposition” is very confusing as mentioned previously; please avoid such shortcut.

- Line 24 of page 28070, I assume the authors mean Figure 3c instead of 3b. Also I would suggest separating the figure from Fig3a-b as it refers to the transition and not to the SAL events anymore.

References:

Delon, C., Reeves, C. E., Stewart, D. J., Serça, D., Dupont, R., Mari, C., Chaboureau, J.-P., and Tulet, P.: Biogenic nitrogen oxide emissions from soils – impact on NOx and ozone over West Africa during AMMA (African Monsoon Multidisciplinary Experiment): modelling study, Atmos. Chem. Phys., 8, 2351-2363, doi:10.5194/acp-8-2351-2008, 2008.

Mari, C. H., Cailley, G., Corre, L., Saunois, M., Attié, J. L., Thouret, V., and Stohl, A.: Tracing biomass burning plumes from the Southern Hemisphere during the AMMA

2006 wet season experiment, Atmos. Chem. Phys., 8, 3951-3961, doi:10.5194/acp-8-3951-2008, 2008.

Saunois, M., Reeves, C. E., Mari, C. H., Murphy, J. G., Stewart, D. J., Mills, G. P., Oram, D. E., and Purvis, R. M.: Factors controlling the distribution of ozone in the West African lower troposphere during the AMMA (African Monsoon Multidisciplinary Analysis) wet season campaign, Atmos. Chem. Phys., 9, 6135-6155, doi:10.5194/acp-9-6135-2009, 2009.

Saunois, M., Mari, C., Thouret, V., Cammas, J., Peyrillé, P., Lafore, J., Sauvage, B., Volz-Thomas, A., Nédélec, P., and Pinty, J.: An idealized two-dimensional approach to study the impact of the West African monsoon on the meridional gradient of tropospheric ozone, J. Geophys. Res., 113, D07306, doi:10.1029/2007JD008707, 2008.

Sauvage, B., Thouret, V., Cammas, J.-P., Gheusi, F., Athier, G., and Nédélec, P.: Tropospheric ozone over Equatorial Africa: regional aspects from the MOZAIC data, Atmos. Chem. Phys., 5, 311-335, doi:10.5194/acp-5-311-2005, 2005.

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