Atmos. Chem. Phys. Discuss., 11, C14301–C14314, 2012
www.atmos-chem-phys-discuss.net/11/C14301/2012/

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.

G. S. Jenkins et al.

gjenkins@howard.edu

Received and published: 16 January 2012

Reviewer # 1 (C12835–C12844, 2011)

We would like to thank the reviewers for all of the helpful comments. We have incorporated as many as possible in the new manuscript.

- 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. RESPONSE This paragraph has been modified an now reflects sources and sinks of 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, ...)

RESPONSE While this would be a nice picture to add, there have been many articles that discuss these features. I believe that the space is better used on results from this study rather than a review from earlier studies. I also have to assume that our readers understand geography and can locate these areas. I have put a star to identify the location of Dakar in Figure 1.

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.

RESPONSE We have identified the dates of profiles associated with the SAL and non-SAL conditions. The criteria is based on the characteristics of the SAL from Carlson and Prospero (1972) – a well mixed layer of dry, warm air with a strong inversion. We have also moved the sentence up to the beginning of the paragraph on how we define the SAL. We have also added the temperature profiles to show that elevated temperatures with an inversion are present for SAL conditions.

- Lines 1-10 of page 28068: The authors state that they use WRF-Chem for compar-

ACPD

11, C14301–C14314, 2012

Interactive Comment

ison 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?

RESPONSE We have added more to the model description, including information as it relates to dry deposition. There is biogenic emissions from the soils and vegetation based on Guenther et al. 1995, but there is no lightning parameterizations in the model. We use the model in a very limited way to only examine the deposition and possible stratospheric intrusion. To go further, would in fact make this a modeling study whereas our goal is for it to be an observational study with a smaller modeling component.

- 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. RESPONSE The monsoon period is not just linked to rain, but it is also linked to humidity, wind directions, AEWs, Squall lines. In this context we have broadly defined what the pre-monsoon/monsoon periods are. We have added a statement, which links the monsoon period to an increased frequency of precipitation events. 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)

RESPONSE

I think that we have clarified the situation in the response before this one and we would like to keep Figure 2d with the group.

-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

ACPD
11, C14301–C14314, 2012

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

RESPONSE We have put in a line to distinguish SAL/Non-SAL and have included it in the Table 1. Furthermore, we define the SAL dates in the text.

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

RESPONSE Does this need to be clarified? There is no doubt that we need to see the spatial and temporal distributions of observed tropospheric ozone. Furthermore, given that we are consider the SAL as an important driver for O3 variations in the vertical (based on earlier studies), we need the AI/AOT to determine if an dust event is impacting our launch site through dust loadings. I believe that we should start out with TCO as we have done since it is the center piece of this work..

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).

RESPONSE Thank you for pointing this out. We have checked over the AI data and found a mistake for the 8 and 10 June data. The old data inadvertently refers to June 9 and 11 rather than 8 and 10. We have fixed it. As for 26 June, even though the AI values are high, it does not really qualify as a SAL event as we now show in Figure 6. Yes there is dry air especially above 800 hPa, but the dew point depressions are low and the inversion is relatively weak, hence we call it a Non-SAL.

-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.

ACPD
11, C14301–C14314, 2012

Interactive Comment

RESPONSE While Backward Trajectories can help Figures 5 and 6 clearly show the flow coming from the North with higher over the Desert for the SAL events. While I favor trajectories I do not think that there is any doubt that the source is from the Sahara; the air is warm and extremely dry in many cases. This would not be the case if it came from lower latitudes where the monsoon would moisten and cool the parcel and the same would be true for the ocean. I believe that Figures 5 and 10 are effective in linking the large-scale flow to AOT.

-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.

RESPONSE We do not have control over the sizing of the figures. I will request to the editor that they send you larger figures. They were larger when we turned them in to ACP. We have added a star in figure 1 to identify Dakar.

-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. RESPONSE You are correct. We do not have NOX measurements and we only suggest that this is the case but it is not a blind suggestion. As stated in the introduction, heterogeneous chemistry is a source of ozone depletion from earlier studies in the published literature.

Section 3.3 -The first sentence should be used in Section3.1 when the onset is defined. RESPONSE Fixed

- 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 trajecto-

ACPD

11, C14301–C14314, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

rieswould 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.

RESPONSE We are not 100% sure of why elevated O3 occurred but NOX enhancement from biogenic emissions seem to be the most reasonable source for the ozone enhancement. The work of Stewart et al. 2008 show significant differences between the dry and wet soils. In figure 7 we now add lightning and 700 hPa Streamline and precipitation. While there was lightning in the region with the two AEWs (especially the 2nd wave), what we can say for sure is that there were several repeated precipitation events prior to the 2 July ozonesonde launch. It is quite possible that some ozone was vertically transported to the lower troposphere via convective downdrafts, especially on June 29th when the precipitation and lightning is oriented in a north-south direction. Modeling this aspect is at present this is outside of the scope of this work for this paper.

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. RESPONSE

We mean an increase with altitude. We have inserted the word.

-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?

RESPONSE

In this paragraph, we focused first on the O3 profiles for the moist cases and last for the SAL event. We then follow it with the Relative humidity profiles. We do not know if they are due to convective transport but we have suggested that (1) 2 July is associated with biogenic enhancement and (2) that 27 August and 3 September are low because of deposition. The SAL case is similar to those in early June. We have also separated the ozone and RH paragraphs.

-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.

RESPONSE We do not have control over the sizing of the figures. I will request to the editor that they send you larger figures. They were larger when we turned them in to ACP. We have added a star in figure 1 to identify Dakar.

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

ACPD

11, C14301–C14314, 2012

Interactive Comment

to reject such stratospheric intrusion event. As a consequencen this modelling study is not convincing to explain the high ozone concentration observed on June 12.

RESPONSE Yes, the configuration of the model and the high ozone concentrations observed on 12 June suggests a stratospheric influence. Even though the relative humidity is high, this is likely associated with the approach of the AEW. We used different initial times to see if the initial conditions, which should be better closer to the event would make any difference. We have added lightning in this section and it now appears that lightning and the production of O3 via LiNOX is the reason for high ozone concentrations in the upper troposphere. We cannot rule out a stratospheric contribution as the model is suggesting. The lower troposphere enhancement is isolated from the upper troposphere based on the wind directions and moisture profiles. The enhancement in the lower troposphere is related to the AEW.

Why the authors have not tested the other hypotheses?

RESPONSE The model was not configured to examine the other hypotheses and this is not a modeling study. In fact, the primary goal of the study was to analyze the ozonesondes during the 3 month period and to report those results. We have not tried to explore all of the parameter space with the WRF-CHEM model as it would take us beyond the original scope of the manuscript. In addition, a significant amount of testing of the model is required especially as it relates to heterogeneous chemistry. For example the model's simulation of dust and the vertical distribution is required when heterogeneous chemistry is turned on. We have been testing this the chemistry and dust modules. The dust module that has the most promise is the GO-CART which we have successfully simulated a dust event in 2010 [Drame et al. 2011]. We are also examining LiNOX in the WRF-CHEM model but this is being tested relative to observations. It will take us additional time to feel confident LiNOX parameterizations at this point. The dust/heterogeneous chemistry/LiNOx simulations are beyond the scope of this study which is weighted towards the obsevations.

ACPD

11, C14301–C14314, 2012

Interactive Comment

- 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.

RESPONSE We now focus on the two profiles from 10 and 12 June only to investigate the lower and upper tropospheric layers. So the figures are confined to the discussion of elevated ozone on 12 June..

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.

RESPONSE We have re-written this section and now imply that LiNOX is responsible for the upper tropospheric enhancement. Modeling all of these factors at this point is outside the scope of this study

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

RESPONSE We have fixed this and explicitly stated dry deposition for ozone.

- 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.).

RESPONSE Please seen new figures 14 and 15 where we show the transport of ozone poor air associated with AEWs and also from southerly winds at Dakar.

- 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 conse

ACPD

11, C14301–C14314, 2012

quence, 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.

RESPONSE This is a mistake. We meant only to focus on the Saunois et al 2009 paper. This paper is not focused on LiNOX or the upper troposphere enhancements. As for the TCO distribution, it makes sense that if ozone concentrations are low in the lower troposphere and transported to the upper troposphere via convection that that lower troposphere is still the source driving the TCO distribution.

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

RESPONSE Our measurements during the monsoon season show that large increases in relative humidity are linked to low ozone concentrations on 27 August. The air masses were from the South and linked to AEWs. The point of showing the simulated O3 and relative humidity is to show that as the monsoon season (higher moisture content) evolves, ozone concentrations trend downward. Our new figures (14 and 15) confirm this and further show that the transport of ozone poor air is linked to AEWs and southerly winds where higher relative humidity is also noted.

- Line 15 to the end of page 28074 with Fig 11: How the model results compare with the observations?

RESPONSE The point of this figure is to show how as RH increases in the lower

ACPD
11, C14301–C14314, 2012

Interactive Comment

troposphere ozone concentrations decrease in the model simulation. We have only 9 observations in this and they clearly show more moist conditions in the monsoon period. Again, we are using the model to evaluate a process rather than to validate the model when compared to a small number of observations.

Please change the x-axis from hours to day of month for clarity.

RESPONSE We have added day as a second axis in the figures

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.

RESPONSE The model is been run in a very limited configuration. The idea is not to do a model evaluation which is a separate study by itself. The idea is to present the observations and to try to understand some of the processes. Because this is one of the first set of ozonesonde measurements in this area we believe that it is important to understand the data with a simple context of the model. That is what we have done. In the context of deposition, and the transport of ozone poor air, ozone, water vapor and meridional winds tell us part of the story. Clear additional chemical measurements are required. We do not claim that we can understand all of the processes in this study with the limited data but we try to do the best that we can.

Section 4 - Lines 2-3 of page 28075: The results stated in this sentence have not been clearly proved.

RESPONSE

I am not sure what you mean. There is clearly an increase in at least four of the SAL profiles. It is not a proof it is just an observation.

- Lines 5-12 of page 28075: Such clear description with altitude range would have been

ACPD

11, C14301–C14314, 2012

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.

RESPONSE We have put in a statement in the first line about the pressure levels associated with ozone reductions. We now put in a statement stating that the depiction of the cartoon in Figure 13 (new figure 17) is based on the results from this work, the SAL aerosol profiles of Ismail et al. 2010 and the results of De Reus et al. 2010. So it is in fact produced from a synthesis of data and results from earlier studies.

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

RESPONSE It could be from either the Sahel or the Sahara but the source of dust impacting these ozonesondes are primarily from the Sahara based on the sources of dust storms. We have to keep open to both possibilities.

- 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).

RESPONSE We clearly state that it is a suggested mechanism. Only new observations can provide us with a true answer.

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

RESPONSE I think that the WRF-CHEM results in Figure 12b do suggest an enhancement of ozone of the order of 30 ppb. Additional WRF-CHEM simulations are beyond the scope of this study. Even if it were possible, there would be considerable uncertainty associated with a LiNOX parameterization at this point.

- Lines 1-7 of page 28077: Idem. This assumption has not been tested. See previous

ACPD
11, C14301–C14314, 2012

comment on the vertical extent of biogenic NOx impact: : :

RESPONSE This is only a suggestion based on the observed data. Additional data must be collected to verify. We are in no way implying that this is the full solution.

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

RESPONSE Fixed

Technical & Minor comments: - Line 3 of page 28064: Please specify “: : : anthropogenic emissions, which act as sources of ozone precursors”. RESPONSE

We refer to anthropogenic emissions associated with industry and transportation.

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.

RESPONSE We have added photolysis

- 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.

RESPONSE Reference added

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

RESPONSE Northwest added C12842 - Line 28 of page 28064: please specify the pressure levels in the SAL features that would help the reader to understand.

ACPD

11, C14301–C14314, 2012

Interactive Comment

RESPONSE The pressure layer has been added.

Line 11 of page 28065, “radicals” instead of “precursors” would be more appropriate. - RESPONSE The word radicals has been added

- 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.

RESPONSE These references were added in the original document.

- 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.

RESPONSE We have added in text to link the Saunois et al 2009 work to Reeves et al 2010.

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

RESPONSE This is fixed.

- 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. RESPONSE We have moved Figure 3c as its own figure (Figure 7).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 28061, 2011.