Atmos. Chem. Phys. Discuss., 11, C14315–C14319, 2012
www.atmos-chem-phys-discuss.net/11/C14315/2012/

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.

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Received and published: 16 January 2012

Reviewer #2 (11, C13113–C13115, 2011)

We would like to thank the anonymous reviewer for all of the helpful comments. We have tried to incorporate as many of the comments as possible in the new text.

However the proposed analysis and discussion could be deeper in view of the paper scientific objectives (objectives 2 and 3 as described in the introduction). While the observations reported are very interesting for the community and possible mechanisms

are discussed, the use of WRF-Chem for observation interpretation and causal attribution does not meet fully our expectations. Consequently, important revisions are still required. Introduction: A better geographical description of the situation of the measurement sites with regards to the main transport patterns as well as chemical sources / sink regions would help to qualitatively appreciate the influence of different air masses.

Section 3. I understood that section 3.1 aims at presenting an overview of the meteorological and surface conditions and their seasonal evolution. However this section also report some findings concerning ozone observations which are then discussed in more detail in the following sections. This is a bit confusing and I would suggest to shift further this discussion as a 'seasonal synthesis' of specific period analysis.

RESPONSE Figure 3.1 has been presented to give a overview of the three month period while providing (1) a monthly overview of the TCO during JJAS; (2) An overview of daily aerosol forcing and daily precipitation near Senegal; (3) An overview of the SAL/NON-SAL calculations. By providing this information up front, the reader is able to get a sense of the overall seasonal changes that occur during the field campaign. We wanted to present this before jumping into the pre-monsoon/monsoon periods.

Section 3.2: L19-21: Heterogeneous chemistry is one factor which could deplete ozone in the SAL but there could be other potential reason. E.g. The SAL air mass could be show low ozone content with or without dust.

RESPONSE It is true that other factors could play a role, but our limited measurements and those associated with earlier published studies conclude that heterogeneous chemistry is an important factor for depletion when mineral dust aerosols are present. Our results are consistent with the earlier studies.

Section 3.3: Pre-monsoon – Monsoon transition. The difference of concentration over the column is quite large. In order to attribute this difference to a production peak of biogenic NO, the authors should rule out any contribution from larger scale transport

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(especially for higher levels). Regional modeling analysis would strengthen the discussion here.

RESPONSE If you examine Figure 7, you will note that the ozone concentrations are slightly lower than in the lower troposphere. We cannot rule out a downward transport of ozone from the free troposphere, but it is clear from Figure 2d, a wet period begins in earnest in the vicinity of Dakar at the end of June, through 2 July. Figure 8 now show lightning and two AEWs which cross the region at the end of June. While there is lightning an increase in surface NOX is expected from the wetting of the soils. It seems logical given that such a pattern is not found later in the monsoon season when there has been numerous rain events but there is no enhancement in ozone similar to what is observed on 2 July. This result is also in line with the measurements of Stewart et al. during AMMA.

Section 3.5: Modelling More information is required on the numerical experiment set up with WRF-Chem: How large is the domain? What are the chemistry lateral boundary conditions? Can that influence the results? Are the different performed simulations a succession of restart to allow ozone field to spin up from the initial state to the analysis period ? Or is it a mini-ensemble an ensemble experiment ? Episode and seasonal simulation are discussed. Does that take part of a same simulation ? I guess a table would be useful for numerical experiment description. In the end WRF is mostly used to assess the importance of stratospheric intrusion. Do we have any insight about the quality of the WRF dynamical simulation (e.g. Easterly wave dynamics) ? To go further, could WRF-Chem be used here to test the contribution of other factor discussed in the paper like soil NO biogenic emission and lightning? (How are they parameterized in WRF-CHem?) Heterogeneous chemistry and photolysis impact of dust are also mentioned in the manuscript. Could WRF-Chem be used to assess the importance of these factors on ozone variability as shown in the observations ? Missing diagnostics, chemistry/ dynamic budgets and sensitivity tests could have brought a lot to this study in order to fulfill the scientific objectives.

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RESPONSE, We have reworked the modeling section. This is done to better represent the simulations and also discuss the lateral boundary conditions. We have made the experiment as simple as possible so that dry deposition and chemical reactions are the only processes that could alter O3 concentrations. There is no lightning parameterization and the gas phase chemistry does not interact with aerosols. Adding these levels of complexity would make this a modeling study, when in fact it is an observation study where simulations are used to help investigate the possible stratospheric intrusion and the reduction of ozone concentrations in the lower troposphere during the monsoon phase.

Figure 17 is added and shows the relationship between AEWs, relative humidity and ozone. There is clearly a relationship in the model, which we have suggested in the introduction. Furthermore, observations from out 2010 experiment confirm that this is the case both in the upper and lower troposphere (Jenkins et al. 2011).

Discussion and summary. The authors emphasize a systematic relative enhancement of observed ozone at the bottom of the SAL, which is an interesting finding. However, the authors draw a conclusion (in fig 13) which is perhaps a bit fast. Author suggest that biogenic NOx are likely to be a source, but the enhancement of ozone at the bottom of the SAL shows up equally at different time in the season i.e. when biogenic NOx emissions are very different. Could there instead be some dynamical reason linked to the SAL, e.g vertical convergence and accumulation of ozone / precursors in the vicinity of the inversion. Again exploring these hypothesis with model production/loss diagnostic would be very relevant here.

RESPONSE The depiction in Figure 13 (New Figure 19) is only a suggestion. The distribution of aerosols, ozone and temperatures are in fact based on observations studies from this work, NAMMA and De Reus et al. (2000, 2005). The question is whether this is a time dependent phenomena. I do not think that it is but more measurements including NOX measurements are required. Measurements from Cape Verde by Lee et al. (2009) show that the highest NOX values that occur throughout the year are from

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the African continent. If we are correct, the bacteria on the dust particles, if they are from the Sahara would denitrify in the presence of elevated water vapor that is mixed upwards from the marine boundary layer just below the base of the SAL. I would suspect that turbulent diffusion would be responsible for the mixing between the layers. If they are from the Sahel, there would be a tendency to denitrify during at periods outside of the wet season. Because dust from the Sahara is most likely during the winter, spring and earlier summer months, both the Sahel and Sahara could serve as sources of ozone enhancement just above the Marine boundary layer.

Lee, J. D., S. J. Moller, K. A. Read, A. C. Lewis, L. Mendes, and L. J. Carpenter (2009), YearâĂŘround measurements of nitrogen oxides and ozone in the tropical North Atlantic marine boundary layer, J. Geophys. Res., 114, D21302, doi:10.1029/2009JD011878.

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