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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 #2

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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.

This paper aims at characterizing ozone vertical distribution variability over Senegal with the ambition of sorting out the role of different chemistry and regional transport mechanisms. This topic is of interest and in line with ACP. A previous version of this manuscript had been in ACPD discussion. Editor and reviewers suggested resubmission with majors revisions, notably concerning clarity, structure and discussion. The

present manuscript improved on the clarity aspect (though minor structural changes are still suggested further). 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.

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.

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

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.

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.

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