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Comment

## ***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**

Received and published: 22 April 2011

“Enhancement and depletion of lower/middle tropospheric ozone in Senegal during pre-monsoon and monsoon periods of summer 2008: Models and Observations.” Jenkins et al.

1. Does the paper address relevant scientific questions within the scope of ACP? Yes
2. Does the paper present novel concepts, ideas, tools, or data? Yes
3. Are substantial conclusions reached? No
4. Are the scientific methods and assumptions valid and clearly outlined? No

C2195

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5. Are the results sufficient to support the interpretations and conclusions? Results are described but not sufficiently analyzed.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Not exactly
8. Does the title clearly reflect the contents of the paper? Yes
9. Does the abstract provide a concise and complete summary? Not exactly
10. Is the overall presentation well structured and clear? No
11. Is the language fluent and precise? No
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes
14. Are the number and quality of references appropriate? No
15. Is the amount and quality of supplementary material appropriate?

#### General comments

The topic of the paper is to explore the causes of elevated ozone concentrations measured by ozonesondes during the summer 2008 in Dakar (Senegal). Measured and simulated results are used to analyse the variability in ozone concentration from pre monsoon to monsoon period. The data are relevant to the problematic. However, the manuscript has several significant issues that must be addressed by major revision before publication in Atmos. Chem. Phys.:

- 1/ The main issue is about the presentation quality: according to ACP quality standards, C2196

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“the scientific results and conclusions should be presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)”. The present paper should first be read by a native English person. Indeed, the scientific content and discussions are difficult to understand, and the analysis is somewhat muddled because of non English syntaxes. Figures are too numerous, and should be combined in a more concise way.

2/ Some references are not always appropriate, and some are often missing to highlight the results.

3/ Description of tools and model are not detailed enough, leading to a difficult analysis of results.

A lot of work is needed to improve the presentation of the paper and to give clear ideas and interpretation. Afterwards, the paper should be more readable and easier to understand. Several specific comments are given in the following, knowing that technical work is first needed to provide a second version of the paper before a second review.

#### Specific comments

Abstract: It will probably have to be rewritten after corrections in the main text.

Introduction OK

2- Observational data and model simulations: English syntax in the first sentence prevents from understanding the main idea. The model is technically described, but not scientifically: we do not know the objectives of the simulations presented in the following, i.e. forecasts beginning at different days and analyzed in section 3.4. This should be explained here to simplify the interpretation of the results. References used to justify the biogenic sources of NO are not appropriate: Guenther et al. (1994) only describes VOC emissions in the United States, and Simpson et al. (1995) gives details on NO emissions from European soils. Have the resulting emissions been evaluated

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or at least examined, to verify the compatibility of such a parameterization on tropical soils? Furthermore, NO emissions from soils are not analyzed in WRF-CHEM results, only the UT/LS layer is approached in section 3.4.

3-Results: This part is very confused. 3.1- Pre-monsoon ozone measurements: Sentences are hardly understandable (English to be corrected). The title is not appropriate because the paragraph speaks about the whole measurement period, and not only about pre monsoon. TCO, AI, AOT and concentration profiles descriptions are mixed, but no real idea or partial conclusion is reached. This paragraph should be re organized with sub sections, with fewer figures and more concise descriptions.

3.2- Pre monsoon/monsoon transition: NO emissions from soils enhance NO<sub>x</sub> and ozone concentrations in the boundary layer (as shown by Saunio et al., 2009, Delon et al., 2008, Stewart et al., 2008). Ozone enhancement in the upper layers should be explained by other mechanisms (long range transport, chemistry).

3.3- Monsoon ozone measurements and simulations: No simulations are described in this part. The concept of “thermal low” is unclear, and should be detailed. The paragraph is mostly descriptive and does not give a clear insight in results. This paragraph has to be rewritten.

3.4- WRF-CHEM simulations...: Title specifies that the case of 12 June will be discussed. Titles should be more general. WRF-CHEM forecasts are presented in this part, without any justification about the modelling procedure. The main idea from these simulations seems to be that stratospheric intrusion enhances ozone concentrations in the upper layer. Surface concentrations should be also commented. Ozone concentrations of 400 ppb are not correctly explained. Upper level concentrations in the WRF-CHEM model do not reach these concentrations, and do not “support lower stratospheric elevated concentrations”. “Guinea northward to the Sahel” is not a correct position. References have to be cited to justify the link between Tropical cyclones Bertha and Ike and AEWs. This last part of the paragraph is particularly confused and

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has to be rewritten.

4- Summary and conclusion: The first paragraph should not be in the “summary and conclusion” part. New figures are introduced and described but should have been introduced above. Heterogeneous chemistry should be discussed in the results part, not in the conclusion. References are missing to highlight the processes discussed here. Figure 13 is not necessary. What are “biogenic sources of NO<sub>x</sub> from Saharan dust”?

Conclusion of the referee is that the paper needs major English corrections and re-organization before being scientifically reviewed.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 7155, 2011.

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