

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: 26 May 2011

RESPONSE TO ANONYMOUS REVIEWER #1

The main issue is about the presentation quality: according to ACP quality “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

C3985

somewhat muddled because of non English syntaxes. Figures are too numerous, and should be combined in a more concise way.

THE TEXT HAS BEEN MODIFIED TO INCREASE READABILITY AND GRAMMAR ISSUES HAVE BEEN ADDRESSED. SEE MODIFIED MANUSCRIPT WHICH IS ATTACHED.

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

REFERENCES HAVE BEEN MODIFIED AND ADDED WHERE REQUIRED

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.

THE MODEL HAS BEEN REFERENCED IN SUCH A WAY THAT IS POSSIBLE TO REPRODUCE THE EXPERIMENT .

Specific comments Abstract: It will probably have to be rewritten after corrections in the main

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

C3986

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.

RESPONSE THIS SECTION HAS BEEN CLEANED UP. THE GUENTHER AND SIMPSON REFERENCES ARE REMOVED AND GUENTHER ET AL 1995 WHICH DESCRIBES HOW NATURAL ORGANIC VOLATILE COMPOUNDS ARE COMPUTED IS ADDED. THIS TECHNIQUE HAS BEEN APPLIED GLOBALLY. IN THE MODEL SIMULATION SECTION AND IN SECTION 3.4 WE DISCUSS THE MANNER IN WHICH WRF-CHEM IS USED. I HAVE EXAMINED THE SOIL NO_x WHICH HAS A MAXIMUM AT 14N AND THIS FALLS OFF TO THE NORTH. HOWEVER, WRF-CHEM IS NOT BEING USED HERE TO EXAMINE SOIL NO_x BUT TO EXAMINE ELEVATED OZONE ON 12 JUNE AND THEN THE REDUCTION OF OZONE DURING THE MONSOON SEASON.

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.

WE HAVE REORGANIZED THE TOPICS INTO RESULTS/PREMONSOON, TRANSITION, MONSOON, WRF SIMULATIONS

3.2- Pre monsoon/monsoon transition: NO emissions from soils enhance NO_x 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).

THIS COULD BE TRUE, HOWEVER IT IS IN THE LOWER TROPOSPHERE THAT SIGNIFICANT CHANGES ARE FOUND IN THE OBSERVATIONS BETWEEN 26

C3987

JUNE AND 2 JULY. THE TRANSPORT OF OZONE FROM MIDDLE TO LOWER TROPOSPHERE CANNOT BE RULED OUT. THE PROBLEM IS THAT WE DO NOT HAVE THE NO_x MEASUREMENTS TO BE SURE THAT NO_x IS ONLY REASON FOR THE ELEVATION OF O₃.

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.

THE SUBHEADING HAS BEEN MODIFIED. THE THERMAL LOW IS THE HEAT LOW. ONE CAN SEE THE CYCLONIC CIRCULATION WHICH IS OVER THE DESERT WHERE TEMPERATURES ARE WARMEST.

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".

THE OBSERVATIONS SHOW A PEAK OF 400 PPB AND THE MODEL DOES NOT CAPTURE SUCH A PEAK. OUTSIDE OF THIS PEAK, OZONE VALUES IN FIGURE 10 ARE NEAR 100 PPB THROUGH 400 HPA. THE O₃ CONCENTRATIONS ARE HIGHER THAN THE 10 JUNE OBSERVATIONS THROUGHOUT MOST OF THE ATMOSPHERE. ELEVATED VALUES ALSO EXTEND DOWNWARD THROUGHOUT THE MIDDLE AND LOWER TROPOSPHERE RELATIVE TO 10 JUNE.

"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

C3988

of the paragraph is particularly confused and has to be rewritten.

WHAT WAS MEANT WAS THE GUINEA REGION NORTHWARD TO THE SAHEL AND NOT THE COUNTRY GUINEA. WE HAVE PUT IN REFERENCES FOR THE TROPICAL CYCLONES. THE PARAGRAPH HAS BEEN MOVED UP TWO PARAGRAPHS.

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_x from Saharan dust”?

WE HAVE CHANGED THE HEADING TO DISCUSSION AND CONCLUSION. IN THIS SECTION WE SUGGESTING THE MECHANISMS OF ENHANCED OZONE WITH SAHARAN DUST EVENTS AND HENCE IT IS A DISCUSSION TOPIC. FIGURE 13 PROVIDES A DEPICTION OF WHAT MAY BE HAPPENING WITH REGARDS TO OZONE VARIATIONS IN THE LOWER TROPOSPHERE. WE HAVE CHANGED THE WORD FROM BIOGENIC SOURCES OF NO_x FROM SAHARAN “DUST” TO “SOILS”. WE BELIEVE THAT THIS IS THE SOURCE FOR ELEVATED OZONE.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C3985/2011/acpd-11-C3985-2011-supplement.pdf>

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