Answer to Reviewer 2 :

Authors are thankful to this reviewer. His/her comments will definitely help to improve the quality of the manuscript.
Below are the answers to each specific points.

Specific Comments:
page 11223 - Although SAFARI and TRACE-A are mentioned in a number of places relevant references are not cited. These include:
D Jacob et al (1996), JGR on TRACE-A
K Pickering et al (1992) JGR on biomass burning mixed with convection; also a TRACE-A paper by Pickering et al, JGR (1996)
A Thompson et al (1996; TRACE-A) - and GRL (2000) on complexities of the tropical Atlantic that pre-date the Sauvage et al (2006) work — this applies to line 21. The RH Brown cruise showed the NH-SH ozone complexity highlighted in the present study. Note that satellite data also bring this to light as in -
Edwards et al (JGR, 2003); R V Martin et al (JGR, 2002? And 2004 or 2005)
Other relevant papers -
G Jenkins - several of these in JGR, ACP

We are sorry for these omissions. We have added most of these references in the introduction of the revised manuscript.
However, the Martin et al. (2004 or 2005) we know about do not seem relevant for our study, as well as the Pickering et al. (1992) wich discuss an urban plume over Brazil.

*page 11225 - line 7. Reference to SHADOZ website *and* to WOUDC woudc.org (World Ozone Data Centre, WMO sponsored) for archive should be provided in addition to AMMA archive*

This was actually said in the introduction (SHADOZ only though). However, for clarity reason it is now removed from the introduction and written in the section 2.1. when we first mentioned the AMMA archive.

*page 11225 - line 24. *and* top of page 11226 -the following are more relevant than Newchurch et al bec they are based on dedicated experiments for looking at data quality critically. Likewise, WMO sponsored work with sonde intercomparisons that has updated the deBacker work, eg:*
Deshler, T., et al., Balloon Experiment to Test ECC-ozonesondes from different manufacturers, and with different cathode solution strengths: Results of the BESOS flight, J. Geophys. Res., 113, D04307, doi: 10.1029/2007JD008975, 2008.
Smit , H. G. J., et al: Assessment of the performance of ECC-ozonesondes underquasi-flight conditions in the environmental simulation chamber: Insights fromthe Jülich Ozone Sonde Intercomparison Experiment (JOSIE), J. Geophys. Res.,112, D19306, doi: 10.1029/2006JD007308, 2007.
Thompson, A M, J C Witte, H G J Smit, S J Oltmans, B J Johnson, V W J H Kirchhoff, F JSchmidlin, Southern Hemisphere Additional Ozonesondes (SHADOZ) 1998-2004tropical ozone climatology. 3. Instrumentation, station variability, evaluation with
simulated flight profiles, J. Geophys. Res., 112, D03304, doi: 10.1029/2005JD007042, 2007b

The reviewer is right. These recent publications are more relevant. We have added these references in the section 2.1 of the revised manuscript.

Page 11229 - Figures 4 and 6 appear to be mentioned out of order

Figures 6 and 9 are mentioned in this section (before figure 5) to give some credit to what

exposed when describing Figure 4. However, Figs 6 and 9 are really described in the following sections.

Page 11226 - on Figure 6, you could compare transport (or at least, winds) to climatology to see if Dec 06 is indeed unusual. Show the reader.

In Dec 06, we actually have only one profile, launched on the 1st. We admit it is then difficult to draw any conclusion on the representativity of this «monthly mean». December is the beginning of the burning season. Thus, it is not surprising to see a significant interannual variability during this month probably more due to the variability and strength of sources (the fire emissions themselves) than to the transport pathways.

Page 11230 - Line 16 - Pickering reference above in SAFARI.TRACE-A issues would be highly relevant.

We are sorry for this omission. Pickering et al. (1996) reference has been added at this place in the revised manuscript.

Page 11235. Line 8 "variability in the Brewer-Dobson circulation" would be better.

This is corrected in the revised manuscript.

Page 11236. Line 3 - NOT SO! Nairobi data (1997-2008 at 1S) are the first equatorial African ozone sounding data with high-density (approx weekly) coverage! In discussing tropospheric columns of ozone, it is surprising that comparisons are not made to the Nairobi SHADOZ record (see Thompson et al., 2003; 2007). This tells quite a bit about meteorological influences and those columns are more accurate than satellite data. The impact of the paper would be greater if the east African data were included.

We understand the comment and the request because we wrote "equatorial Africa" where we meant "West Africa". We apologize for this misunderstanding. We have made the revised manuscript clearer regarding this issue. It is written West Africa everywhere now.
Our study deals with West Africa data only recorded in the frame of AMMA. The paper is an overview of the ozone sounding data over Cotonou. Therefore we do not think it would be appropriate to include the East African data (Nairobi) in this study. It may be the subject of another study. Nairobi and Cotonou data sets are very different, at least in the lower troposphere. They have different seasonal characteristics and different background concentrations. Globally, Nairobi shows lower ozone, because there is no influence of biomass burning in the lower troposphere (no harmattan, no AEJ). See Figure 1 below. Tropospheric columns of ozone are also lower than over Cotonou, as highlighted in Figure 2 below. This is partly due to the missing first 2 km. Besides, they do not show the same seasonal cycle as over Cotonou. Figures 1 and 2 highlight these differences.

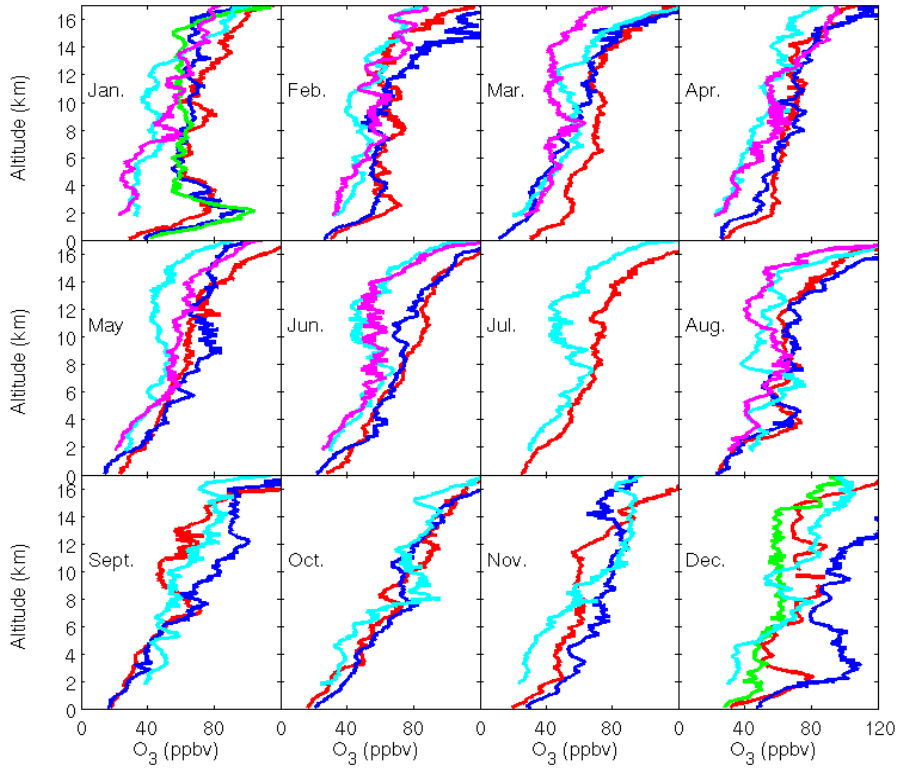

Figure 1 : Monthly mean vertical profiles of ozone from the Cotonou RS (2005 in blue, 2006 in red, Dec 2004 and Jan 2007 in green) and from the Nairobi RS (2005 in cyan, 2006 in magenta) from the surface to 17 km altitude.

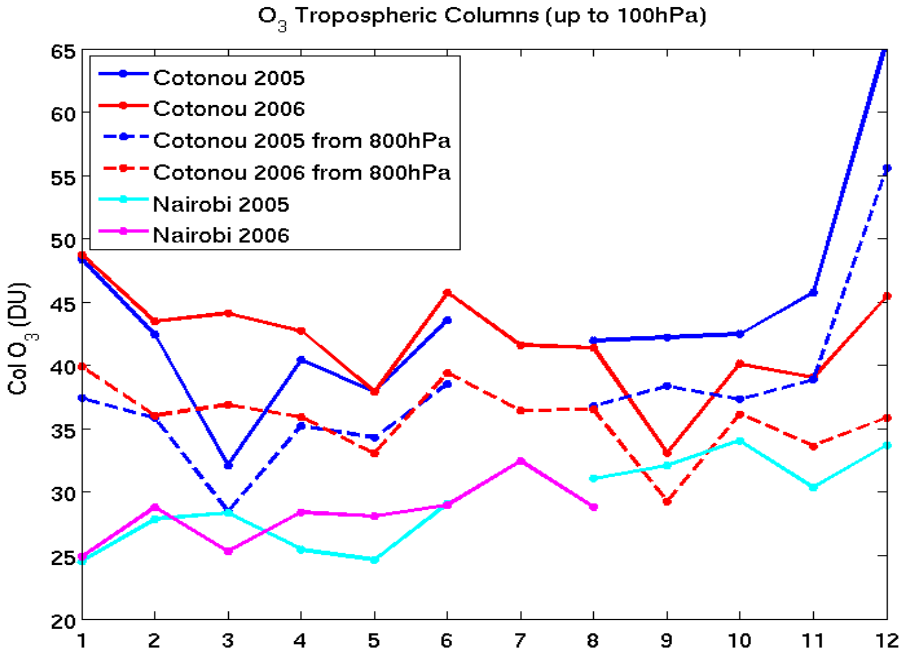

Figure 2: Seasonal cycle of the monthly mean tropospheric columns of ozone in DU from RS over Cotonou (2005 in blue, 2006 in red; dashed lines are for the same years but the columns are calculated between 800 and 100 hPa only) and RS over Nairobi (2005 in cyan, 2006 in magenta).

Page 11237. Line 26. September month; (needs semi-colon)

This is corrected in the revised manuscript.

Page 11239. On this page there is much discussion of values like 4.99 DU, 1.66 DU -these seem as if over-stating statistical significance of figures. What is the precision one really expects? Also applies to page 11240 esp line 26.

The reviewer is right. We do not expect a 0.01 DU precision. We will consider only one decimal in the revised manuscript.

Line 15. More omission of prior work. In addition to Pickering et al., JGR (TRACE-A, 1996), Smyth et al, 1996 reported lightning influences on elevated NO in tropical southern hemisphere. Levy and Moxim (2000) is an important modeling reference, albeit for early lightning season.

We are sorry for this omission. These references have been added in the revised manuscript, in section 5.1.

Page 11240. Even in the tropics, where the tropopause is relatively "firm" upper tropospheric air shows signs of stratospheric influence. The various products referred to attempt to find a more accurate tropopause but layers of stratospheric and tropospheric air intermingle and cannot really be discriminated often within the TTL,"tropical tropopause layer."

It is not clear for us. We do not understand this comment. We agree with this statement but we do not know what the reviewer want us to do.

Figures 2&4 - the read vs magenta is not distinguishable to many readers

Colors of these figures have been changed by red and blue instead of red and magenta.

Figure 8 - impossible to read

Figure 8 has been completely redone as also suggested by reviewer 1. It is now clearer in the revised manuscript.

Figure 14. It seems significant that the poorest agreement of the OMI/MLS product occurs during the highest soundings. What is the effect of averaging?

The reviewer is right. The comparison is obviously difficult due to the different horizontal and temporal resolutions. We said that these 2 sources of discrepancy may explain most of the differences between soundings and OMI/MLS as highlighted for December 2005, page 11241, lines 4-6.