Atmos. Chem. Phys. Discuss., 3, S2234–S2236, 2003
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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Space-borne observations link the tropical Atlantic ozone maximum and paradox to lightning" *by* G. S. Jenkins and J.-H. Ryu

**Anonymous Referee #1**

Received and published: 18 December 2003

I have previously reviewed (anonymous reviewer #1) the paper "Linking horizontal and vertical emissions of biomass fire emissions to the Tropopical Atlantic Ozone Paradox during the Northern Hemisphere winter season: climatology" herafter called "paper 1", which appeared previously in ACPD. I have been asked by the editor to review this second paper, with some extra attention to issues of duplication.

This paper follows a similar approach as paper 1, combining information from a weather forecast model, with results from satellite derived tropospheric ozone columns, lightning data from OTD-LIS, and fire counts from ATSR. New to this paper is the use of Shadows ozone sonde data.

Like paper 1 the authors skillfully combine several pieces of satellite, model and



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measurement information to make a qualitative case that lightning- and not biomass burning- is responsible for most of the tropical Atlantic ozone maximum. It convinced me, especially the summary/discussion section integrates and interprets a number of previous studies to make the case.

However, as in paper 1, the problem is that the study is very qualitatively written, whereas previous studies at least tried to put some numbers on the influence of lightning. Again this paper should show better, where they reach a new conclusion, based on data interpretation, and where they confirm previous studies.

As to the overlap with paper 1, there are quite some. The same kind of satellite data are used, presented a bit differently, but essentially the same. Paper 1 attempts to demonstrate that biomass burning and consequent transport cannot explain the ozone maximum, and suggests that lightning is the more likely candidate. This paper goes more in detail on the lightning part. Strangely paper 1, did not mention that a similar paper was to be submitted, nor does this paper refer to paper 1.

I recommend that the authors try to focus on the really new parts of the two papers, and try to condense them in one paper for ACP, the readers interested in the background information will find their way to the original papers in ACPD.

Detailed comments: p. 5728: a short description of the differences between the TTO and CCD tropospheric ozone methods, and why they produce these differences would be useful, especially because at p. 5729 l. 9 you suggest that the difference between TTO and CCD should tell us something.

All Figures: the graphical quality is not very good; in my printed version I had difficulties to read legendas etc.

All Figures: in the Figure captions it would be good to mention the data source.

p. 5744 figure 2 and 3 are essentially the same thing.

Fig 10/11/12/13: Fairly hard to read: would it be possible to leave out some pressure



**ACPD**

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Interactive Comment

levels, and focus on the other altitudes?

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5725, 2003.