Atmos. Chem. Phys. Discuss., 3, S1896–S1898, 2003
www.atmos-chem-phys.org/acpd/3/S1896/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Linking horizontal and vertical transports of biomass fire emissions to the Tropical Atlantic Ozone Paradox during the Northern Hemisphere winter season: climatology" *by* G. S. Jenkins and J.-H. Ryu

Anonymous Referee #2

Received and published: 14 November 2003

In their paper, the authors present a detailed analysis of the Tropical Atlantic Ozone Paradox using data from many sources, mainly satellite instruments and meteorological models. Their main findings are that

- tropospheric ozone in the Northern Hemisphere biomass burning season DJF is reduced as vertical transport by deep convection is suppressed and thereby ozone lifetime reduced
- tropospheric ozone in the Southern Hemisphere is at the same time enhanced by NO_x production through lightning in both South America and Central Africa

- that this is true not only for case studies already presented by previous authors, but also in a climatological sense.

The paper provides a thorough discussion of many processes involved in the build up of tropospheric ozone in the tropical Atlantic region and a useful collection of data from many data sources. Although most of the data is available in the internet, the combination of the different data sets and the presentation in form of climatologies is a useful reference for scientists working in this field.

However, in my opinion the paper is in most parts limited to a description of existing data and the discussion of previous work, and it is not clear to me, what the new findings of this study are. The interpretation of the data is very qualitative, and possible problems associated to the different measurements are not discussed. For example, the ozone columns derived from TOMS measurements by alternative methods differ systematically, indicating that the retrieval has a significant impact on the results and their interpretation and can not be ignored.

The discussion of the impact of lightning (which is a central part of the paper in spite of the title that only mentions transport and biomass fire emissions) is particularly weak: that lightning could be a source of upper tropospheric ozone has been discussed in many publications, both qualitatively and also quantitatively using models. The graphs presented in this study (OTD/LIS flash rate climatology and NCEP winds) demonstrate that there is strong lightning activity over South America and Central Africa, and that any ozone or ozone precursors produced there could be transported over the Atlantic. However, this has been pointed out many times before, and the important questions

- how much NO_x is being produced?
- how much ozone would this produce?
- are these numbers consistent with the ozone columns and profiles observed from space and ozone sondes?

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are not even mentioned.

It is also my impression, that while previous work is extensively discussed in the paper, it is not always clear how much of what is said is actually a summary of the results of previous studies. For example, on page 5077, l22 it is stated that “these conclusions are supported by the results of Martin et al. (2002) and Edwards et al. (2003)”, while in fact most of the points have already been discussed in these papers and are only iterated here.

In summary, the paper provides a useful overview over the topic, but in my opinion does not contain many new results. The authors should stress the differences to previous work, reduce the description of well known data sets and try to make the arguments less qualitative.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5061, 2003.