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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new ENSO index derived from satellite measurements of column ozone" *by* J. R. Ziemke et al.

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Referee #4 Comments on Title: A new ENSO index derived from satellite measurements of column ozone Author(s): J. R. Ziemke et al. MS No.: acp-2009-852

General comments

The manuscript introduces a new ozone ENSO index in tropospheric column ozone derived from satellite measurements covering a time period from 1979 to present.

The work is complete, conclusions are clear, the text is well written, the methodology is well presented. Anyway, I would recommend publication subject to minor revisions, as detailed below.

Thanks for these constructive general comments of the paper.

Main concern

The manuscript could be shortened, as the effective new information could be summarized in few figures (see below for details). I would suggest to better put in evidence what is really new with respect to previous works [other than different/longer time-series].

It is not clear what the analysis on the SCO variability from GEOS-CCM model is relevant for. I suggest removing it from the ms.

All of the figures with discussion including those for the CCM represent new science. The recent Aura MLS ozone measurements in our paper are much better than the SAGE, HALOE, and UARS MLS [used by Ziemke et al., 1998] for evaluating the variability of SCO in the tropics. Ziemke et al. [1998] could only derive rough estimation of the zonal variability because of the poor quality of SCO measurements at the time. (This is discussed in the paragraph beginning on line 154.) To our knowledge, the CCM results presented in this paper is the first model results which demonstrate the zonal invariant property of SCO. We feel that the paper should not be shortened and that inclusion of the CCM results greatly strengthens our paper.

Specific comments

Section 3.1 and discussion of Figures 1,2,3 could be shortened [maybe producing one single figure], as the main result [low east-west variability of tropical SCO] has been already recognized in a different study [Page 5, lines 120-122: "THIS ZONALLY . . . "]

Section 3.2, Figure 5 and the analysis on the GEOS-CCM seem not relevant for the manuscript. Moreover, why the discussion about the assimilation of winds? If GEOSCCM reproduces the QBO as a spontaneous mode of variability [good characteristic of the model], why justifying that the assimilation is not used?

These points along with our reply are essentially the same as those above. We do not understand these statements regarding the GEOS-CCM.

Figure 8 shows the correlation between CCD TCO and Nino3.4 and SOI. How different is this information w.r.t. to Figure 3 Ziemke and Chandra GRL 2003 [the TCO/ENSO regression]?

Figure 3 of Ziemke and Chandra [2003] is a contour plot of regression coefficients whereas Figure 8 in our paper is a contour plot of correlation which has a very different physical meaning and statistical evaluation. Also, Figure 3 of Ziemke and Chandra [2003] used tropospheric ozone from a different derivation method, a smaller latitude range (15S-15N), and very different time window (1970-2001).

I think that the most interesting information is in figures 6,7,9,10, with really new results in figure 9-10. The rest of the manuscript could be shortened.

We agree with you that these figures are interesting, but again we do not wish to shorten the paper of figures or text. Shortening the paper would only dilute the manuscript of completeness and scientific content.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2859, 2010.