

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

Anonymous Referee #4

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Review of : A new ENSO index derived from satellite measurements of column ozone, by Ziemke, J. R. et al.

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.

Main concern

C1179

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.

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 GEOS-CCM reproduces the QBO as a spontaneous mode of variability [good characteristic of the model], why justifying that the assimilation is not used?

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]?

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.

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

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