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## ***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

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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.

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

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