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

J. R. Ziemke et al.

Jerald.R.Ziemke@nasa.gov

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Referee #1 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

This paper defines a new ENSO index using satellite measurements of total column ozone, based on the finding that total column ozone variability in the tropics results mainly from variations in tropospheric ozone, which varies systematically with ENSO. The paper is well written and clear, and the analysis methods are fully explained. It is a suitable topic for publication in ACP, and, to my knowledge, it represents original work. However, I raise a few concerns below that I feel should be addressed before publication to improve the value of this contribution to readers.

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Thanks for the comments. They have been very helpful in revising the manuscript.

General Comments

1. The motivation of this work is not clear. Although the method is clever, what is the purpose or value of a new ENSO index? Why is it important to have an index based on tropospheric ozone of a phenomenon that involves interactions between the tropical ocean and atmosphere and that is manifested in meteorological and sea surface conditions? The idea that this might be useful for evaluating climate models needs better justification. Has any modeling study ever identified a need for such an index?

We emphasize in the revision that a well calibrated ozone ENSO index is important not just for long-term monitoring of tropospheric ozone and its photochemical and radiative effects relating to ENSO, but also as an important diagnostic tool in aiding the development of models of the troposphere including free running GCMs. We note that the CCM used in our study to evaluate stratospheric ozone is currently being extended for the inclusion of a troposphere with inter-working dynamical winds, ocean temperatures, and photochemistry. The CCM modeled tropospheric ozone and other trace gases are not yet useful for quantitative study. Models such as the CCM should be capable of reproducing the ozone Pacific dipole in the troposphere and its dynamical relationship with sea surface temperature and surface pressure. In the revision we discuss these points in detail.

2. The reliance on satellite observations both limits the potential period of record and misses an opportunity to evaluate the tropospheric ozone variability associated with ENSO. Did the author's consider using in situ ozonesonde or Umkehr data to assess directly the ENSO signal in tropospheric ozone?

We do not understand these comments. The satellite measurements have nearly continuous spatial coverage in the tropical low-latitudes and extend over a much longer time record (1979-current) than ozonesondes. Several papers have been written previously (some included in the reference list) on the subject of ENSO and ozonesonde

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versus satellite measurements of tropospheric ozone. Ozonesonde measurements from SHADOZ include Watukosek in Java and have been shown to agree well with TOMS and OMI measurements which include ENSO events.

3. The idea, expressed in the abstract, that the spatial differencing procedure used to compute the new index is an important aspect of this work is a bit misleading. Many ENSO indices (including the classical Tahiti-Darwin surface pressure difference) are based on spatial differences, as are many indices of other modes of climate variability.

This was mentioned to emphasize that the ozone ENSO index from satellite retrievals is not affected by inter-instrument calibration offsets or instrument calibration drifts over time. For satellite measurements using multiple instruments to produce a long-record ENSO index this is a critically important property.

4. The paper introduces a bunch of new and unnecessary acronyms that confuse the reader, rather than make the paper easier to read. For example, TCO is meant to be tropospheric column ozone, but I misread it more than once as total column ozone. AO is used for annual oscillation, but is easily read as Arctic Oscillation. For ozone, I suggest using subscripts (strat, trop, total) to avoid confusion, and avoid other new acronyms as much as possible. The abstract, in particular, is full of undefined acronyms, which should probably be spelled out.

The revision has eliminated some acronyms including OLR, SST, and AO, but retained those that are used many times in the manuscript such as TCO and SCO for “tropospheric column ozone” and “stratospheric column ozone”, respectively. (SCO is used about 90 times and TCO is used about 40 times in the manuscript.) For “total column ozone” we have retained spelling it out in the revision without an acronym. This use of acronyms for ozone is consistent with our previous papers.

5. The paper is also full of jargon that is probably familiar to specialists in satellite observations of ozone, but is baffling to the rest of us. Specific examples are given below.

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The revision includes clearer discussion of the satellite observations of ozone.

6. Finally, the authors have focused on ENSO, which is but one of several modes of global climate variability. Have they also considered developing ozone-based indices of other modes, such as the Northern (or Southern) Annular Mode? These extra-tropical modes might be even more interesting to examine in the context of understanding dynamical contributions to ozone variability.

You are correct that many indices besides ENSO have been developed. In the tropics there is also an “Indian Ocean Dipole” (IOD) index and a MODOKI index (based upon three regions in the Pacific rather than two). There is also a “Pacific Dipole Oscillation” (PDO) index and an “Arctic Oscillation” index, both involving extra-tropical measurements. Our study has focused on developing only an ENSO index with CCD measurements which is limited to tropical latitudes.

Specific Suggested Changes

The Introduction would benefit from inclusion of references to classical ENSO literature and to papers dealing specifically with the issue of developing indices for complex modes of climate variability. Also, it would be good to cite references showing a relationship between ENSO and trace gas distributions.

The current manuscript lists references on these points including classical ENSO and ENSO effects on other trace gases, but the paper does not discuss them in detail. For conciseness, under the central theme of deriving an ozone ENSO index, we have limited such discussions in the manuscript. Other trace gases besides ozone such as CO and H₂O are shown in the referenced studies to exhibit marked changes in the Pacific associated with ENSO events.

L53 – “obtained via the internet” is not a sufficient reference for datasets.

We agree to some extent with this comment, but satellite data sets these days almost invariably come directly from websites or anonymous ftp (usually described from these

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same websites). We have opted not to change this in the revision.

L56 – Doesn't CO₂ also show an ENSO signal? And is this related to atmospheric changes or to carbon fluxes to/from the tropical ocean?

It would be interesting to evaluate CO₂ in relation to ENSO but this is beyond the scope of our study.

L96 – Define “level-2”

We have clarified the level-2 footprint measurements in the revision.

L111 – “TOMS webpage” is not a sufficient reference.

We again agree to some extent with this comment regarding website references, but the only existing reference for the current TOMS algorithm comes from the TOMS ATBD website.

L112 – What is “the v8 algorithm” – a recipe for a canned beverage?

Version 8 includes many modifications from version 7 including improved a priori tropospheric ozone, an aerosol and sea glint correction, an improved tropospheric efficiency correction, and an offset correction for Earth Probe TOMS measurements. We mention these points in the revision and leave more detailed discussion of the version 8 algorithm to the referenced literature.

L113 – What are “UV cloud pressures”? Section 3 needs to discuss the precision and accuracy of the observations since the results are given to the ~ 1 DU level. Are the data reliable to that level? Section 3 (\sim L118) should also discuss the horizontal resolution of MLS data, and the implications for detecting an ENSO signal. Does the large MLS “footprint” impact the spatial differencing?

The UV cloud pressures are discussed in Section 3 of the revision. Section 3 also details the inferred uncertainties in the measurements for TOMS, OMI, and MLS, including MLS measurement path length.

L148 – Sentence is unclear. Is deep convection or low boundary layer ozone the explanation of low ozone concentrations in the troposphere?

A combination of both deep convection in the presence of low concentrations of ozone in the boundary layer/low troposphere was inferred by Ziemke et al. [2009] as a likely cause for the low ozone concentrations measured inside clouds over the remote Pacific.

L163 – The 5 DU zonal changes in stratospheric ozone should be compared with mean values to give the reader a sense of their importance; perhaps percentage changes would also be informative.

This 5 DU uncertainty corresponds to 5 DU out of about 230 DU mean SCO amount in the tropics (~2%). The Aura MLS measurements in the current study indicate that this number in monthly means is considerably smaller. The revision also expresses the percentage number.

L171-2 – Is this an assertion, results from prior research, or results from this study?

The revision now mentions Ziemke et al. [2005] on this point.

L210 – “updated” from what, and how?

There have been no updates to the atmospheric GCM since Reinecker et al. [2008] so we have changed the text to clarify this point.

L216 – “excessive eddy transport” - Please define the type and scale of the eddies in question.

This discussion of Tan et al. [2004] in the revision has been re-written for clarity.

L225 – Why was 1979-94 the period of simulation?

The simulation was continued to 2008 using observed sea surface temperatures and sea ice concentrations; however, it was only done up until 1994 at time of submission.

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We have updated the text to reflect this. The specific period that was highlighted in Figure 5 is typical of any years selected between 1979 and 2008.

L235 – Isn't a third factor the small zonal variation in ozone production and destruction?

We added text to mention this third possible factor. "A third possible factor is that there are very small zonal variations in the production and destruction of ozone in the tropics."

L242 – Is the use of Dobson Units/km standard, or are DU usually used for (even defined as) total column units? I'm not familiar with this way of expressing an ozone gradient.

This is a common method for representing vertical gradients of trace gases - by simply adding the column-gradient contributions with altitude (DU/km) one can easily determine the physical abundance (molecules per unit area) for a given altitude/pressure window in the atmosphere.

L239-252 – Perhaps this whole paragraph is unnecessary, as the "important result" stated in the final sentence is actually already discussed in the preceding text.

Most points mentioned in this paragraph were not stated in previous discussion in Section 3.2. This paragraph has been modified in the revision.

L258 – Why choose Oct 2004-mid 2009? Was this the period of maximum overlap of the datasets?

This represents the time period of Aura for which we can compare these two coincident SCO measurements.

L285 – Should the 1986-87 event be included in the list?

An El Nino event occurred in 1986-1987, but it had a small effect in altering tropospheric ozone in the tropics compared to the other mentioned El Nino events dating back to the beginning of the TOMS measurements (i.e., 1979).

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L293 – Please provide details of the “data mining method”, so that others might be able to reproduce the results. So much detail is given about other aspects of the analysis that this obscure part stands out. Section 5 Summary could be dramatically shortened by providing a more succinct summary of the results and avoiding repetition of material from other section.

The referral to “data mining” has been deleted in the revision.

L366-7 – Is this shown in the paper? If not, either give a reference or avoid speculation.

We now include reference to the Chandra et al. [2002] paper.

L378 – In addition to the NASA web page, the dataset should be published in ACP along with the article. It is a fundamental result of this work and so should be published along with the methodology.

Our tropospheric and stratospheric ozone data have been made available to the public since 1997. The merged total ozone data set has been made available to the public since 2004. These ozone data sets are updated periodically with extended records and new version algorithms. Because of these changing factors it may be best to only reference the NASA TOMS website regarding the data.

Fig 1 caption should include time period and contour spacing, as some contours are illegible.

This has been implemented in the revision.

L531-2 repeats material in main text unnecessarily.

It is okay sometimes to repeat a statement in a figure caption from main text if it is an important/useful point and makes the figure caption more complete.

Fig 7 time axis should be in 4-digit years and should not be labeled “Month”. Also, caption should be clear about whether these are model or observational data.

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Appropriate changes have been made in the revision.

L571-3 not needed in figure legend.

Our response to this comment is similar to above for Figure 5.

Figs 9 and 10 could be combined.

We have thought of this, but opted to have them remain as individual figures.

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

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