

Interactive comment on “Winter- and summertime continental influences on tropospheric O₃ and CO observed by TES over the western North Atlantic Ocean” by J. Hegarty et al.

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This paper gives a comprehensive classification of winter summer TES ozone and carbon monoxide measurements according to defined meteorological conditions. It is well referenced and clearly written. I recommend publication after minor corrections.

General comments: 1) The paper is well organized, but too long. Any compression of sections 5 and 6 would be helpful, but it is hard to make specific suggestions.

We plan to reorganize the manuscript combining Section 3.1 with Section 5 and Section

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3.2 with Section 6. We will also eliminate Table 1. We anticipate that these changes will make the paper shorter. See response to Referee 2 for more details.

2) The authors refer several times to the climatological a priori used by TES as introducing artificial structure. In fact, this variable a priori allows TES to retrieve O₃ more accurately, especially in the tropopause region, which changes significantly with latitude and is where TES has somewhat lower sensitivity (and therefore retrieved profiles have more contribution from the prior). Although I agree that a common prior should be substituted when an analysis is trying to examine spatial or temporal variability due only to measurements, it does not necessarily produce an optimal retrieval, especially over a large latitude range. I have tried to address this in the specific comments with changes to words such as “artifact”. Please plot or list the vertical profiles of the assumed universal priors that were used for CO and O₃ and say something about the assumed tropopause for O₃ (i.e., whether or not it is appropriate for these mid-latitude cases.) For CO, you could include the a priori profile in Figs 7b, 9b.

We will include some additional information about the universal a priori used. The decision to reprocess with the universal a priori is a difficult one for all of the reasons you mention. However, for our study it was important to capture/emphasize the real measured differences associated with the different synoptic patterns and seasons and this was facilitated by reprocessing with the universal a priori.

3) There is no discussion of the TES uncertainties that are reported with the data. In the case of seasonal averages (large N) the error could be assumed to be the error of the mean (σ/\sqrt{N}) and might be small enough, i.e., within a color bar, for the distribution plots. (If this is the case, it needs to be stated somewhere). However, for the O₃-CO correlation, retrieval errors can be important, as shown in Zhang et al., 2006. This could be referenced specifically.

We have added a brief description of the composite distribution creation which briefly addresses the TES uncertainties in the colored composite plots at the beginning of

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Section 4 in the revised manuscript

We have included a citation to Zhang et al., 2006 in Section 4.1 concerning the uncertainty in the O3-CO correlation. See response to comment 19.

4) Please add a list of acronyms and make sure all are defined at the point they are first used (e.g., WCB, DA, PCF).

We will add this list in a new table.

5) Figure captions should refer to the tables where the map types are defined.

We will add these table references to the figure captions.

6) Global replace Ederling with Eldering.

I apologize for the error and it has been corrected in the revised manuscript.

Specific comments: 7) Abstract – lines 35,36. Change wording to: “: : reprocessed to remove the influence of the a priori on geographical and seasonal structure, exhibited strong seasonal differences.”

Change made to manuscript.

8) Abstract line 43: Are the O3-CO slopes 0.15-0.20 or 0.15-0.23? (as in Sec. 4).

The slopes of 0.15-0.20 listed in the abstract are correct. The slopes listed in Section 4 have been corrected in the manuscript.

9) Abstract line 57: Include a short summary sentence at the end of the abstract, e.g., “For both seasons, we demonstrate clear associations of enhanced CO and O3 measurements with corresponding meteorological conditions.”

Change made to manuscript.

10) Section 1 Intro: line 93 - define DA

The description dry airstream has been added before DA, and DA will also be defined

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in a new table of acronyms.

11) Section 2.1 TES Data. TES CO data products after Dec. 2005 have higher sensitivity due to better optical alignment after a change to the optical bench operating temperature. This might not be that important here since you are not looking at interannual variability, but since you include specific cases from June 2005 (in figure captions, but not text), you might want to include this in the data description.

The figure captions referring to the June 2005 were incorrect. The cases were in June 2006. For further clarification we added the following statement to Section 2.1.

“Since TES CO data products after December 2005 have higher sensitivity due to better optical alignment resulting from a change to the optical bench operating temperature (Osterman et al., 2007a) we investigated only synoptic and interseasonal variability and not interannual variability.”

12) Section 2.1 lines 147-149: wording change to: “Because the climatological a priori adds spatial and temporal structure that could potentially obscure the real variability we would like to examine, (Zhang et al, 2006), we removed this a priori structure by reprocessing: : :”

Change made to manuscript.

13) Section 2.1, line 151, include reference to S. S. Kulawik et al., ACP 2008. (as already included in the online comments) on the validity of swapping the a priori.

Citation added to manuscript as suggested in online comments.

14) Section 2.1, line 151, maybe in this paragraph, as mentioned above, please show or list the universal a priori O3 and CO profiles that were used.

We will include the universal a priori profiles as an additional figure in the revised manuscript.

15) Section 2.2, line 178 – edit “of the use of the use”

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Change made to manuscript.

16) Section 2.2, Explain the GDAS and EDAS modes for HYSPLIT.

We added the following explanation to the end of Section 2.2 to the revised manuscript:

“Meteorological inputs to HYSPLIT were from the Global Data Assimilation System (GDAS) (Derber et al., 1991; <http://www.arl.noaa.gov/ss/transport/gdas1.html>) at a resolution of $1^\circ \times 1^\circ$ and from the Eta Data Assimilation System (EDAS) at a resolution of 40 km (<http://www.arl.noaa.gov/ss/transport/edas40.html>).”

17) Section 3.2, line 289 – change “descending DA airstreams” to “descending dry airstreams”

Change made to manuscript.

18) Section 4: line 329 – give a definition of “significance level p ”. Do you consider measurement errors in this?

The significance level was determined using a standard t-test. The significance test just shows the degree to which there is a significant linear relationship between the two independent variables O₃ and CO; however, measurement error is not explicitly accounted for. We suspect that random measurement error would possibly weaken any real linear relationship, as suggested in the next comment, so this test probably underestimates the significance.

I have added the clause “determined using a t-test” after “significance level of $p=0.01$ ”.

19) Section 4 line 331 – could add reference here to Zhang et al, 2006 about how TES retrieval errors will also tend to reduce the observed correlation.

We added the following text after “(Daum et al., 2006)” in Section 4.

“Though the TES slopes are on the lower end of the range observed in previous studies, TES retrieval errors tend to reduce the observed correlations (Zhang et al., 2006).

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Moreover, care must be taken when comparing . . .”

20) Section 4: line 340 – “one representing and active” should be “an active”?

Change made to manuscript.

21) Section 5.1 lines 415-418 – to examine the possibility of higher/lower sensitivity at 618 hPa, you could look at a map of the TES AK diagonal at this pressure. (just a suggestion - not a requirement for this paper)

That is a good point. I did plot the vertical profiles of averaging kernel diagonals for the and found that for the points with enhanced CO at 681 hPa they generally peaked near 0.15 at the 908 hPa level, and remained high (> 0.10) up until about 681 hPa before decreasing more rapidly to a minimum value at 425 hPa, thus indicating good sensitivity to CO from the top layers of the marine boundary layer to higher altitudes in the lower troposphere. For the points with lower CO the diagonal values were generally at 0.05 or less from the surface up to the 681 hPa and then increased to a single peak value in the upper troposphere indicating decreased sensitivity to lower tropospheric CO.

22) Section 5.1 line 423 – “: : instruments such as TES are less sensitive to CO” – Don’t you mean “more sensitive”?

Yes, it should read “more sensitive”. Correction made to manuscript.

23) Section 6.3 line 664 – Text refers to June 2006, but figures 22,23 have June 15, 2005. Please make these consistent.

The text is correct it should be 2006. The figure captions have been corrected.

24) Section 7 line 699-702 change to “While the climatological apriori used in the TES retrieval process may add temporal and spatial structure to the retrieved distributions, the observations were reprocessed with a universal a priori to remove this effect”.

Change made to manuscript.

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25) Tables 1a, 1b, horizontal lines separating Map Types would be useful.

Lines added to Tables 1a and 1b. However, per suggestion of Referee 2, these tables will be deleted from the revised manuscript.

26) Tables 2 3. What is the significance if a or b is not given? Also, Tables 2 3 could be combined.

All the other slopes in the tables were not significant above the 90We will consider combining the two tables, but they may be easier to read as two separate tables, especially considering the fact that there would be 2 "All" rows.

27) Figure 1. As in general comment, refer to table 1a for definitions. Also helpful to define WCB, PCF, DA, W2 in the figure caption.

We will include these references and definitions in the figure captions.

28) Figures 3, 4 – Could use common color scale for CO (b, d) maybe saturating at 125 ppb.

We will try plotting the CO with a common color scale so that the seasonal and vertical differences will be easier to see. I did try several color scales and settled on those that covered the dynamic range in both the seasonal and individual synoptic pattern distributions. However, I will revisit this issue and perhaps there will be a good compromise between the two color scenarios.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23211, 2009.