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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Winter- and summertime continental influences on tropospheric O_3 and CO observed by TES over the western North Atlantic Ocean” *by* J. Hegarty et al.

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This review is by Owen Cooper, a co-Editor for ACPD/ACP and the editor of this manuscript. Two anonymous referees have provided very thorough and insightful reviews of this paper and I thank them for the obvious time and effort they put into their write-ups. In addition to the concerns raised by the referees I list below some additional points and concerns that need to be addressed during the manuscript revision.

Some references need to be checked: 1) Wotawa and Trainer, 2000, addresses just biomass burning smoke transport but it is used as a reference for STE.

2) Polvani and Esler, 2007, deals with idealized cyclone transport and does not specif-

ically address STE over the western North Atlantic. Given the context of the Introduction, this reference should be removed.

We removed these references from the list on page 23213, line 16.

3) Cammas et al., 2008, discussed intrusions of tropospheric air into the stratosphere but I didn’t find any mention of stratospheric intrusions impacting the lower troposphere of eastern North America or the Atlantic.

This reference did mention biomass burning signatures over Washington DC at 4km, 5km, and 7km on page 5833 (in more recent final ACP version Cammas et al., 2009) and on page 5834 it states

“Whatever sections of MOZAIC observations shown on Figs. 2and 3 are chosen, the source contribution comes invariably from North West America (Alaska, Yukon and North-West Territories); hence maps of biomass fire CO source contributions (Fig. 7) are shown only for the MZ1 observations and the MZ2 observations in the free troposphere during the descent to Washington DC.”

However, on closer inspection it looks like they only confirm mixing with stratospheric air for the upper troposphere above 11 km so we will remove this reference from the list on page 23213, line 16.

We will also update the reference to Cammas et al., (2009) later in the manuscript when it is cited for a more general discussion on the contributions of fires to continental export.

4) Owen et al. 2006 do not discuss decreasing cyclone activity in summertime compared to other seasons.

We have removed this reference from the list on page 23214, line 21.

5) Parrish et al 2000 did not discuss the PCF. The concept of the PCF was introduced by Cooper et al., 2001 and expanded in Cooper et al 2002a.

Change made to revised manuscript.

page 23214 line 19 When discussing the decrease in summertime cyclone activity do you mean intensity, frequency, or just a shift to the north?

Actually all three occur according to the climatological studies cited. We have changed the revised manuscript to read "synoptic cyclone activity over the mid-latitudes decreases in frequency and intensity as the preferred storm tracks shift to the north".

page 23218 line 3 Should this be 2004-2006?

We actually did the correlation training on a longer set of meteorological analyses to ensure that the selected patterns were climatologically relevant and not just anomalously common during the two years for which we had TES data. This information was unfortunately deleted from this version of the manuscript in our internal editing process. We intend to reorganize this section based on the comments of Referee 2 and combine it with Section 5. As part of the reorganization we will include a separate introductory section that describes the synoptic classification procedure with a little more detail.

page 23219 line 2 While the PCF can consist of air that descends from the mid-troposphere to the boundary layer, it can also just refer to the low level boundary layer flow behind a cold front that doesn't necessarily descend.

We changed the revised manuscript to read "The PCF airstream is the boundary layer flow behind the cold front that in some cases has descended from the mid-troposphere".

page 23222 line 5 A reference is needed for the O3/CO slopes from ICARTT 2004.

These slopes were shown in Zhang et al., 2006. We added this reference to the revised manuscript.

page 23223 DJF3 has a low centered over NY State. Region 1 is mostly ahead of

where the WCB should be located, so I am not convinced that this region experiences WCB transport. This is probably why very few trajectories show ascent.

Many of the trajectories at least show southwesterly flow into this region and the sea-level pressure composite shows some kinking of the isobars off to the east of the low center suggesting secondary cyclone development. Thus we suspect that some of the CO enhancement could be due in part to the WCB.

page 23224 line 15-20 Similar to the comments of one of the referees regarding skew-T's, I recommend you use the coastal skew-T plots to infer the atmospheric structure over the water. Then take a typical sea surface temperature and allow it to ascend along wet adiabats (assuming the sea surface air parcel is saturated) to see how high it would rise. Does this agree with the cloud top heights that you can estimate from GOES IR images and the skew-T, or model temperature profiles? Also, how high does a polluted air parcel need to be lofted before it has a significant impact on the 681 hPa TES retrieval?

We will include the coastal skew-T plots suggested by Referee 2 and change the text of the revised manuscript accordingly.

There is no easy answer to your last question. Because of the broad vertical weighting functions (i.e. coarse vertical resolution) of TES and the correlation between adjacent levels inherent in the a priori any substantially enhanced mixing ratios at or above an altitude with sufficient thermal contrast with the surface may be sensed by TES and impact the retrieval at that level and several levels above. The extent to which levels are impacted is dependent on both the absolute mixing ratios and vertical distribution and it is often difficult to distinguish which factor is more important. For example a band of extremely high CO mixing ratios confined to a shallow band may appear to TES to be nearly indistinguishable from a band of moderately enhanced mixing ratios spread over a greater vertical depth. That is why we used satellite images showing bands of convective clouds and skew-t soundings to estimate the depth of the vertical mixing.

Ultimately one might use a high resolution chemical transport model with accurate parameterizations of vertical mixing processes along with retrieval simulations to more accurately estimate the transport from the boundary layer and the contributions from continental sources. While such investigations are beyond the scope of this paper they are precisely the types of studies that we hope our paper can inspire.

page 23224 line 20-21 Here you say descending but in Figure 9 you say ascending.

The text is correct it should be descending. We corrected the figure caption in the revised manuscript.

page 23228 line 8-10 Here you talk about CO accumulating in the stratosphere, but are these particular retrievals actually in the stratosphere? Please compare these retrievals to PV plots to determine if they are above or below the tropopause. It would also be helpful to plot ozone vs. CO for these cases and look at the scatter plot distribution. Does the distribution look similar to the stratospheric samples in Figure 6c [Cooper et al., 2005] which shows ozone versus CO from all Northern Hemisphere MOZAIC flights above 1 km during February 2004? Cooper, O. R., et al. (2005), Direct transport of midlatitude stratospheric ozone into the lower troposphere and marine boundary layer of the tropical Pacific Ocean, J. Geophys. Res., 110, D23310, doi:10.1029/2005JD005783.

From the composites it looks like the regions of enhanced CO correspond roughly to areas of 400 hPa PV of 1.5 -2.0, which suggests some stratospheric influence. However, we will look in greater detail at the PV plots for these particular cases and compare the O3 and CO scatter plots to those reported in the suggested reference. We will report any relevant findings in the revised manuscript.

page 23228 line 10-14 Arctic haze is primarily a lower tropospheric phenomenon, so your trajectories that show transport to the upper troposphere of the Arctic are not likely tapping this pollution source A good paper on Arctic residence times is: Title: Characteristics of atmospheric transport into the Arctic troposphere Author(s): Stohl A

Source: JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES Volume: 111 Issue: D11 Article Number: D11306 Published: JUN 9 2006

We have deleted the reference to Arctic haze from the revised manuscript.

page 23230 line 1 Do you mean northeastward rather than northeasterly?

Yes we meant northeastward. We corrected this word in the revised manuscript.

page 23230 Here a boundary at 50 N is discussed, but to my eye it looks more like 45 N.

The references to 50 N have been replaced with 45 N in the revised manuscript.

page 23230 Sea breezes are offered as a possible lofting mechanism, but are typical sea breezes strong enough to loft pollutants to 681 hPa? Please find references that describe observations (not model results) of such deep transport.

Due to the coarse vertical resolution of TES, pollutants may need to be lofted only to 2km or lower depending on thermal contrast to impact the retrieval at 681 hPa. While according to Roland Stull "An Introduction to Boundary Layer Meteorology" the depth of the sea breeze circulation can range from 500 – 2000m, I was not able to locate any observational evidence of sea breeze circulations as deep as 2 km along the coast of the U.S. as most topped out between 500 – 800 m. However, it is conceivable that convective mixing could have lofted pollutants to the top of the daytime convective boundary layer which ranges from 1.0- 2.5 km over land during the summer, and then some of these pollutants were transported offshore. Enhanced O3 mixing ratios > 95 have been observed by Angevine et al. (2004) (see complete citation in manuscript) at altitudes as high as 950 m a.m.s.l in the coastal regions of New England, but unfortunately lidar observations were not reported above that altitude (Angevine, et al., 2004).

We have changed the manuscript to postulate this mechanism rather than the sea breeze.

page 23230 What is the mechanism to reduce the O3/CO slope in Region 3? Please see Real et al. (2008), ACP, 8, 7737-7754.

Considering the conclusions reached by Real et al., (2008) and upon closer inspection of our composites and O3/CO slopes it appears that transport to Region 3 was not very likely during the times when these synoptic patterns persisted. Therefore we have modified the revised manuscript to read.

"However, under this scenario it may take up to 10 days for an air parcel to cross the Atlantic Ocean (Owen et al., 2006). Therefore the somewhat lower CO and O3 mixing ratios (Figures 18a, 18c, 19a, and 19c) and near zero O3-CO slopes (Table 2b) in Region 3 possibly reflect the reduced likelihood of transport across the Atlantic Ocean in the time frame of several days for which this synoptic pattern typically persisted. This is supported by back trajectories that come from the mid- and upper troposphere over North America or from the south and east from lower altitudes over the ocean (Figures 20a and 20c)."

page 23232 line 1-2 What causes ascent in northerly flow behind a cold front? Do satellite images show convective cloud?

This should read "southerly flow" (northward) and has been corrected in revised manuscript.

page 23232 I am confused how convection over the central US influences CO and O3 over the Atlantic at the exact same time. Please provide additional explanation.

The transport occurs over several days, but the synoptic pattern is persistent over that time period as described in the text.

page 23234 In the summary it would be helpful to compare the average summer and winter export patterns to your previous springtime results. Of the 3 seasons the greatest O3 export is in summer at around 35 N which is an important result to convey. Your average summertime results should be compared to the recent modeling results

of: Fang, Y., A. M. Fiore, L. W. Horowitz, A. Gnanadesikan, H. Levy, Y. Hu, and A. G. Russell (2009), Estimating the contribution of strong daily export events to total pollutant export from the United States in summer, J. Geophys. Res., 114, D23302, doi:10.1029/2008JD010946, 4 December 2009

We will include these comparisons in the revised manuscript.

Figure 3 4 and 11 the numbers on the color scale are too small, please enlarge

We will enlarge these numbers in the revised manuscript.

Fig 4 We don't need to know the stratospheric ozone mixing ratios in detail for this study. Please change the color scale to increments of 5 ppbv that focuses on the range of tropospheric values.

We will change the color scale to 5 ppbv to emphasize the tropospheric variability.

Figure 5 Please overlay the location of the TES retrievals, add a coastal outline map and increase the contrast of the image.

There is a problem with adding the map background to the images obtained from the NOAA CLASS archive that has not yet been resolved by their technical support staff. If it can't be resolved soon we will attempt to plot the raw pixel data ourselves and add the map background and TES observations.

Figure 7 and 9 Please increase the size of the skew-T by at least a factor of two.

We will try to increase the size of these plots, though the final size will be determined by the typesetting performed at the production office.

Figure 16 17 and 20 Please increase the size of each panel

We will try to increase the size of each panel but we are limited by the page width. We could try to plot them all within a vertical column, but they may not all fit on one page. Alternatively we will try to plot them in landscape mode.

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