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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Synoptic influences on springtime tropospheric O_3 and CO over the North American export region observed by TES" *by* J. Hegarty et al.

J. Hegarty et al.

Received and published: 26 December 2008

Anonymous Referee 2 Received and published: 28 November 2008

I have a fundamental problem with the major premise of this manuscript. The authors assume that if they composite six different patterns of sea level pressure, that distinct patterns of CO and O3 will result. This approach does not consider the different higher altitude flow patterns that can occur within a given cyclone composite. This approach also does not consider directly how the various airstreams are located with respect to sources of pollution. The result is that the CO and O3 patterns for the six classes do not have clearly different patterns, and the authors have difficulty explaining these chemical patterns. There is a great deal of speculation about what leads to each pattern

(WCB, location of pollution sources, etc.). And, the manuscript8217;s summary barely mentions the six categories, but instead focuses on the overall six category composite along with a few specifics. I get the impression that the authors have attempted to cover too much material (overall seasonal, specific map types, individual cases), with most of the discussions turning out to be unconvincing.

The objective of this study is to utilize TES to understand continental pollution export. Continental pollution export unequivocally originates via dynamical processes influencing the continental and adjacent marine boundary layer. In this study we deployed the map typing approach to identify these dynamical processes. The unique characteristics of each map type resulting from our analysis clearly suggest distinct positions of the cyclones or anticyclones in the geographic domain and by extension their relative positions with respect to the continental pollution sources. Because of this fact we hypothesized that we could show differences in the TES O3 and CO distributions related to circulation type. Heretofore analysis of TES data, which is relatively new, has focused mainly on global and seasonal distributions and we therefore feel that our work is a valuable addition to the verification of this instrument.

We applied the map typing technique to the sea level pressure (SLP) fields. As discussed in the manuscript in Section 2.3, we used the SLP fields because geopotential height fields tend to become smoother with height and the correlation between maps becomes dominated by the main north to south gradient, leading to misclassification of circulation types. We also pointed out that using the cyclone positions from the surface analysis one can locate the general position of the major cyclonic airstreams or lack there of in the case of anticyclonic systems. We do not ignore upper level features but rely on the fact that in the mean these features will be related to the surface features. For example, one would expect that developing mid-latitude surface cyclones would be located in the downstream side of an upper level trough where warm air advection, positive vorticity advection, divergence, and lower tropospheric warm air advection promote cyclonic development. More mature cyclonic types would tend to

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be more vertically stacked with the upper level trough centered right above the surface cyclone. In contrast to cyclones, anticyclones would be located on the upwind side of an upper level trough. We had created composites of the 700 hPa and 300 hPa fields using the SLP types to classify the days and found, as expected, that this was true.

I would have approached this topic differently. I would begin with cases of TES-derived patterns of CO and O3 related to East Coast mid latitude cyclones, calculate backward trajectories from specific chemical features within each, and then group the trajectories based on the sources (or lack of sources) that were encountered. There may be other approaches that would yield a similar result. Unless the authors can convince me that their approach is valid, I cannot recommend publication of this manuscript. Specific issues are noted below.

There are most definitely alternatives to tackle a scientific problem, and undoubtedly the reviewer is entitled to theirs. However, all approaches have their inherent caveats, and ours is not exceptional. The limitation imposed on our study is that one cannot establish a *pattern* of CO and O3 distribution with any one particular cyclone. The reason is that, as described in Section 2.1, TES observations are only available along the TES orbits which are separated by approximately 20 degrees longitude and only available over a given region once every 12 hours (the ascending and descending orbits of a Global Survey) and then not again for another 2 days. This means that only a small portion of any particular cyclone is sampled. Therefore, it is necessary to establish the composite spatial patterns of TES O3 and CO observations associated with cyclones in similar locations and stages of development. The map typing provided a method of identifying these cyclones (and also anticyclones) and classifying the time periods of these systems so that the observations could be grouped to construct the composite.

There are inevitably some subjective decisions that need to be made in map typing, such as correlation coefficient, domain size, and location. However, even an approach based on selecting East Coast cyclone cases would rely on subjective criteria such as the cyclone central pressure perturbation, size (radius of last closed isobar), position, and vertical depth. Furthermore, while cyclonic airstreams are certainly important for continental export, it is also important when evaluating the ability of TES measurements to capture variability caused by synoptic systems to show the distributions that might result when cyclones are not located along the east coast. Evaluating the distributions for a variety of map types, not just cyclones, will help establish whether the enhancements of O3 and CO are really associated with the unique flow characteristics of the cyclone, are due to some other mechanisms, or are an unexplained artifact of the TES retrieval process.

We agree with the reviewer that back trajectories can be useful in identifying sources. However, in our opinion, relying entirely or primarily on back trajectories can also unequivocally bring about substantial uncertainties. The critical vertical velocity fields are modeled and not measured and while they should do a reasonable job of representing the grid scale where the meteorological analysis is accurate (i.e. over the continent), relatively minor errors in the meteorological analysis could result in large errors in the vertical motion field. In addition, the coarse meteorological data (40 km to 1 Œ 1 degree) may not resolve many of the smaller-scale vertical motions critical to the continental pollutant export. Therefore a back trajectory approach may also be open to subjective interpretation and should be combined with other synoptic evaluation. That is the very reason we have combined trajectory and synoptic analyses in our case studies as discussed in Section 6.0. In addition, as suggested by the editor we created back trajectory composites for selected regions for each of the map types and they generally support our speculated transport scenarios. We can include these in the revised manuscript.

1. page 19748, line 28 and elsewhere8212;8220;681 O38221; This is a poor grammatical way to express O3 at 681 hPa. It is a type of lazy grammar.

We have changed this expression to 681 hPa O3 here and in all other places in the manuscript.

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2. top of page 197508212;It is not clear why you used a universal a priori field. Specifically your sentence, 8220;The geographically variable a priori adds artificial structure, which can potentially obscure some of the real geographical variability of a trace gas.8221; I thought one purpose of the a priori was to help the retrieval process produce variability that it otherwise would not detect because of limited vertical resolution in the sensor and the retrieval algorithm. I don8217;t understand your thinking here. Please explain it better in the text.

One should not add variability that is not really there or detectable with the a prior. The a priori is necessary to constrain the solution which is mathematically not unique. It is advantageous to have an a priori, which for TES O3 and CO is also the first guess, that is climatologically representative of a region because in general it will set the retrieval in a linear regime and speed, and in some cases insure convergence. However, if the signal from O3 and CO is very weak due to meteorological factors or even the trace gas distributions themselves the retrieval will be heavily weighted toward the a priori. Since the a priori varies geographically it can add artificial (mainly horizontal) variability and obscure some of the real variability. The use of a universal a priori is also discussed in Zhang et al., 2006 referenced in manuscript and also in Kulawik et al., 2008, www.atmos-chem-phys.net/8/3081/2008/. We will add more detail in Section 2.2.

3. page 19751 line 128212;Please better describe HYSPLIT8217;s ensemble approach8212;specifically the 8220;shifting of the met fields by one grid point8221;. Does this amount to picking a location and then creating multiple trajectories around it and in the vertical?

Yes, that is the correct interpretation. Our description is similar to what is on the HYSPLIT website, but we can add your last sentence in the affirmative if that would help.

4. page 19751 line 68212;When I have used the Lund (1963) technique, I required a correlation of at least 0.7. Did you investigate whether requiring a greater correlation

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would produce better results? Keep in mind that it is better to leave some cases uncategorized than to have too many variations within a given category. How many of your cases were left uncategorized?

We tried correlation coefficients between 0.5 8211; 0.7. Our criteria were to be able to classify >70

Just as an aside, we have found that we need to use higher correlation coefficients in the summer than in the spring possibly due to the more subtle nature of the synoptic scale features in summer.

Lines 10-158212;Although the GPH upper level fields are 8220;smoother and less distinct 8221; that SLP8221;, subtle differences in upper level flow play a crucial role in determining transport. This gets back to my opening paragraph. Your assumption on lines 14-15 is a HUGE one, and I do not believe it is sufficiently valid.

We do not dispute that upper level flows play a crucial role or that the upper level flows are different form the surface. However, over a mean of a number of cases there are general relationships between the upper level and surface fields, as discussed earlier in this response, and therefore the surface fields can be used to classify the overall synoptic circulation type.

5. Fig.28212;These panels would be much easier to interpret if you would label each center with an H or an L.

We will add these labels to Figure 2.

6. page 197538212; Although this section attempts to distinguish between the various map types, I do not believe that I could duplicate your categorizations because your criteria do not appear 8220;hard and fast8221;. Instead, there appears to be considerable subjectivity in the categorizations e.g., in distinguishing MAMS 2-5. Please describe your criteria more specifically, perhaps by modifying Table 1 or adding a new table. The more 8220;hard and fast8221; your criteria are, the greater your chances of

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having CO and O3 patterns that are explainable.

The major differences are in the locations of the cyclones and anticyclones and how that may impact the flow along the east coast and adjacent Atlantic Ocean. The hard and fast criteria are the map typing parameters which control the categorization of maps.

If I am correct, the unique feature of MAM1 is its semi-stationary nature. However, I note on page 19765 that you labeled a case persisting from 9-15 May as MAM3. This confuses me. Also by first discussing the various airstreams when describing MAM2, you give the impression that MAM1 does not have these airstreams (i.e., WCB, etc.) Was that your intention? Doesn8217;t MAM1 also have these airstreams in most cases?

We found that MAM1 cases tended to be persistent particularly in early spring as described in the manuscript, and we also did identify one case of MAM3 that was persistent in late spring. In spite of that one particular persistent MAM3 case, the overall patterns of the two types had distinct differences which were unmistakably diagnosed by the map typing algorithm. In general, MAM3 patterns feature a cyclone centered south of Long Island and produce easterly 8211;northeasterly flow along the coast of the northeast and westerly- southwesterly flow from the southeastern U.S. to the Atlantic. In contrast, the MAM1 cyclone is centered east of the Canadian Maritimes with north-northwesterly flow along almost the entire U.S. east coast, these factors which are evident in the SLP patterns are what are depicted in the map typing algorithm.

We had no intention to imply that MAM1 systems don8217;t have WCBs or DAs etc. Our intent was to suggest that the position of this cyclone would have isolated it8217;s WCB from recent influence by the major polluted regions of the continent. Furthermore because of the persistent nature of this system there would potentially be greater mixing with other airstreams over time thus reducing the spatial variability of the distributions as compared to MAM2.

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You describe types MAM2-5 in the last two paragraphs of Section 3 and refer to Fig. 2. Some of the MAMs look very similar in Fig. 28212;just some small displacements. So, as you point out, their histories were very important in the classification process. Nonetheless, even cyclones having the same track do not necessarily have the same vertical structure such that their trajectories would be the same.

The differences are in the locations of the cyclones and anticyclones. While there are differences between individual cases we would expect there to be general trends in transport pathways related to the map types. Our newly constructed composite back trajectories show these general trends.

Finally you mention that the CCB often is a very cloudy region. However, the same can be said of the WCB where it overrides the CCB in the vicinity of the warm front. In fact, the classical airstream model shows greatest WCB ascent in the warm frontal area

Yes, that is true, but we are looking for areas further south in WCB and possibly in the secondary WCB where clouds are fewer and retrievals are more likely (see Section 6 and Figs 10 and 11).

7. Throughout the text you use 8220;elevated8221; in places where I think you mean 8220;enhanced8221;. The text would be more specific if you reserved 8220;elevated8221; to references to altitude. This especially is problematic in those sentences that describe both altitude as well as concentration.

We will change all references to elevated O3 and CO levels to 8220;enhanced8221; in the manuscript.

8. Fig. 38212;MAM6 has the greatest PV. Any thoughts as to why this occurs? MAM1 has an even lower SLP, while MAMs 2 and 4 have the same central pressure (1004 hPa). Of course, SLP alone is not the sole indicator of PV, but that gets back to my major concern with your methodology.

We do not believe these points are highly relevant to the context of our discussion. We

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have not correlated the intensity defined as central pressure of SLP with the O3 and CO distributions, and we discuss PV only in terms of defining regions of stratospheric tropospheric exchange.

However, two points are noted here if they are of help to clarify the argument. First, MAM6 depicted more compact cyclonic circulations which tended to be more mobile and usually in an earlier phase of life cycle than the MAM1 cases which were semi-stationary and usually in a stagnant or decaying phase. Second, MAM2 and MAM4 had the same central pressure, but MAM4 had only 1 closed contour while MAM2 had 3, and as we all know, the central pressure is not the only measure of a cyclone8217;s intensity.

9. page 19757 line 138212;There is no Fig. 7g; it should be Fig. 6g.

Change made to manuscript.

10. Tables 1 and 2 and corresponding text8212;What was your total number of cases? Also, it would be helpful if the total for each map class would be added as a column in Table 1. Table 2 has them listed by regions, but an overall total for each MAM would be useful.

The total number of maps classified as a given type is included as a percentage of the total (180 days) in Table 1. If the reviewer was referring to the total number of O3 and CO observations for each map type we can include those also.

11. I have no problem with Fig. 4 and its discussion. It is the individual map types that follow that are a concern.

We don8217;t understand the point of this comment. Please be specific.

12. page 197568212;Please succinctly state your reasons and criteria for defining Regions 1-3. You state that Region 1 contains the greatest CO, but what were the criteria for defining the other two regions? Later when you describe the individual map types in Table 2 and the text, the lat/lon bounds for the three regions do not vary with

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map type. So, for example, do the lat/lon bounds for MAM1 always contain the greatest CO8212;even though the low centers are not at the same locations? This confuses me.

We defined the three regions in which the characteristics in the seasonal means of O3 and CO were somewhat similar. For example Region 1 is immediately off the coast with both CO and O3 enhanced, Region 2 also close to the coastline with enhanced O3 and decreased CO, and Region 3 with an eastward extension of enhanced O3 out into the central Atlantic but decreased CO. We did not vary the regions by map type so that we could compare the characteristics in each region across map types. For a given map type, for example MAM1, the low centers would be in approximately the same location. If the reviewer meant to ask 8220;do the lat/lon bounds for Region 1 always contain the highest CO8221;, the answer would be no. The CO levels would depend on the type of circulation pattern and its interaction with the regions of pollution sources.

13. There is a great deal of speculation (8220;likely a result of8221;, 8220;suggests8221;, 8220;may be8221;, 8220;may have potentially8221;, etc.) about the causes for the various CO and O3 features in the six map types. I suppose this is the best you can do based on your categorization methodology, but that gets back to my major objection to this approach.

We would use these expressions of uncertainty regardless of the methods employed. There is a great deal of uncertainty in all analytical methods, as we pointed out in our response to reviewer8217;s second comment. The ensemble trajectories in this manuscript (Figs 10, 11, 13) serve a confirmation of this point. They clearly suggest that a 1-3 grid point analysis error can result in dramatically different path ways and this doesn8217;t even take into account the unresolved vertical motion features. Hence caution should be taken in interpreting model-based products. Furthermore there are uncertainties in the TES data. While TES can distinguish between upper and lower tropospheric features it cannot decisively identify the exact altitude of a particular feature. Therefore even if the trajectories were perfect the source region could be arguably

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uncertain.

Our analysis is a first step to examine whether there are any substantial trends or characteristics of the O3 and CO distributions related to circulation types that are observable by TES. As more TES data is accumulated and models and analysis fields improve future analyses may be able to reduce and quantify the uncertainty.

14. page 19759 line 17 and Fig. 88212;At 316 hPa, doesn8217;t one expect that the northern third of your domain will be dominated by the stratosphere (whether or not a cyclone is present)? This will have a major influence on your correlations. Shouldn8217;t this be mentioned?

Yes, that is true. We will make the distinction between the enhanced O3 levels north of 50 N and those to the south in the revised manuscript.

The changes will be 8220;were poleward of 50 N, which is expected given the lower tropopause height at those latitudes (Fig. 8b and c). The exceptions were areas of enhanced O3 extending well south of 50 N near the 8230;8221;

According to classical cyclone theory, the WCB will not have transported air to the 316 hPa level until considerably north of the warm front8217;s surface position. This would generally occur northeast of the low center. The bottom of page 19759 does not make this point clear.

We will replace the last sentence of this paragraph with:

8220;According to the classical cyclone theory, the WCB will not have transported air with fresh east coast emissions to the 316hPa level until considerably north of the warm front surface position, which would be northeast of the low center and probably outside our study domain.8221;

15. page 19760 line 188212; 8220;the cyclones were displaced from the coast by anticyclones8221;. This is not good meteorological wording. Reading farther, why should this result in significant contributions from the WCB? The DA contribution is *easier to understand since more of the DA region is within your lat/lon domain, but I do not understand the WCB aspect. It is very difficult to determine your major points in Section 4.3*

We will change manuscript to read:

8220;in that anticyclones were located along the coast and cyclones were located off-shore resulting in ..8221;

16. Section 4.48212;The center of the surface low in MAM6 appears to be farthest out to sea of any other map type. Doesn8217;t this fact alone have a major influence on the chemical concentrations and patterns?

Here we are trying to make a distinction between the other cyclone patterns which are closer to the coast and are associated with higher O3 and CO concentrations at the 681 hPa level. Since MAM6 is well out to sea the pollutants would also be further out to sea and the levels near the coast, where an anticyclone restricted lofting to the free troposphere, would be comparatively lower than the other types.

17. Last para. of Section 4.48212;This is a very telling paragraph. Assuming that the manuscript can be made viable, this information also needs to be stated at the beginning of the results sections

This paragraph is a lead into the next section and perhaps it might be better if moved to the beginning of Section 5. In addition we will paraphrase some of these points after the last paragraph in Section 4.0 where we discuss grouping the TES retrievals by map type. This will alert the reader to the fact that we will discuss both composites distributions and individual case studies.

18. page 197638212;Your first case study represents a fairly deep, but compact cyclone. Yet, the IR image does not reveal a major cloud band associated with the WCB. Adding a VIS image to Fig. 10 might be helpful here since it hopefully would show the lower level portion of the WCB. It would not require extra space since you currently

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have a gap where the fourth panel would be placed.

The archive to which we had access only had IR imagery. We will try to find the visible image from other archives.

19. page 197648212;Based on Fig. 11, it appears that approximately half of your trajectories might represent the secondary WCB due to their path and low level origin. However, the other half appears to begin at a high altitude and begin northwest of the low center. This suggests a dry intrusion. The fact that they passed over large emissions while approaching the low and associated frontal region could explain the CO and O3. So8230;you might have a combination of the two airstreams. This is interesting and could be presented in greater depth.

Yes, this is some of the uncertainty related to trajectory analysis. A small shift in starting location will give a totally different source location. In fact, we looked at some trajectories from just one-two hours earlier that were even more promising, showing trajectories crossing the boundary layer of the New York City Metropolitan area before being lofted to the free troposphere and then re-circulated around to the back of the cyclone.

While there were no ozonesondes launched that day, one could look for data from other instruments such as MOPITT, AIRS or OMI to see if we can follow the pollution plume. Another option is to use a chemical transport model, but that would be outside the scope of this current analysis.

20. Section 6 has a weak start. Please provide a better theme sentence.

We will change the statement to read:

8220;The TES O3 and CO distributions indicated that the polluted continental air masses modified as they traveled further out to sea.8221;

21. page 19765 line 258212;I am still puzzled as to why a cyclone lasting from 9-15 May should be categorized MAM3 instead of MAM1

There are major differences in these circulation patterns as discussed in response to comment 6.

22. Section 68212;I believe your goal here is to present a case of enhanced O3 over the Atlantic in the lower troposphere that suggests continued O3 production. However, I am not sure what your final conclusion is. Your trajectories are interesting, but what have you proven? It appears that much more is needed.

We were attempting to explain how 681 hPa CO levels could decrease noticeably out to sea while O3 levels remained high. We postulated several mechanisms including dilution and photochemical processing of CO and further photochemical production of O3 (page 19765 lines 9-12). We also demonstrated using trajectories that anthropogenic pollutants may be diluted in this region by mixing with middle and upper tropospheric air. We have since created back trajectory composites which show that this is a region of mixing of many different air masses and we can include them in the revised manuscript.

I wish you the best with revising this manuscript.

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