Atmos. Chem. Phys. Discuss., 3, S1669–S1675, 2003
www.atmos-chem-phys.org/acpd/3/S1669/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intercontinental transport of tropospheric ozone: A study of its seasonal variability across the North Atlantic utilizing tropospheric ozone residuals and its relationship to the North Atlantic Oscillation" *by* J. K. Creilson et al.

J. K. Creilson et al.

Received and published: 7 October 2003

Response to Referee Comment S1509:

We appreciate and thank the insightful and constructive comments brought forth by Parrish of our paper. Our responses to the major specific comments and minor comments and technical corrections that Parrish discusses are addressed below. Responses to comments which require additional analysis or increased discussion will be reflected in the final version of our paper.

Major Specific Comments:

1. Major Comment #1: The issue of anthropogenic versus stratospheric influence as the cause of the spring ozone observed at Bermuda is still a subject of lively debate. He would like to see an expanded discussion on this subject, as well as a more detailed discussion of stratospheric-tropospheric influence in general.

Response: Based on previous comments by Stohl (Referee Comment S1314), we have discussed the expansion of the section of the paper that addresses stratospheric-tropospheric influence (Section 5). The focus of the expanded discussion centers on the potential vorticity (PV) analysis performed during a high ozone/positive springtime NAO episode over Region 5. We have agreed to include this analysis as part of the expanded discussion. We also discussed the Sprenger and Wernli (2003) paper and how their results relate to our findings. As part of the expanded stratospheric-tropospheric section of the paper, we will include the Sprenger and Wernli (2003) reference as well as the discussion that was part of our Stohl response. In addition to the expanded section as a result of the Stohl comments, we will address the remaining questions within this comment. Specifically, we will re-examine a specific subset of the data and decide if we can further add to the issue of stratospheric-tropospheric influence at Bermuda. Looking at climatologies that are developed for these areas, such as the Sprenger and Wernli (2003) paper, a climatological determination will be made of the magnitude of influence it may have during a positive NAO year. If the analysis provides meaningful results, we will add these results to the final paper. In regards to the last comment in the paragraph, we agree that the east coast of North America can be an area of enhanced stratospheric-tropospheric exchange during non-summer months. Specifically, the winter and early spring are time periods when the polar jet stream can typically be found in the mid-latitudes. However, our strongest relationship is one that occurs during a positive phase of the NAO. During this time the westerlies are further north and a more zonal flow pattern has set up. This pattern tends to induce less troughing with higher height anomalies over the North Atlantic, causing less of a chance of stratospheric in-

ACPD

3, S1669–S1675, 2003

trusion into the troposphere. Also, during the spring season, we found that the months of April and May are driving our strong spring correlation, not March. And since the meridional flow patterns that drive some of the stratospheric-tropospheric exchange events are climatologically more prevalent in the winter and early spring, we feel that the tropospheric ozone we are seeing in the spring is more likely due to photochemical activity and subsequent advection. We will, however, examine Sprenger and Wernli (2003) to determine the climatological location of east coast stratospheric-tropospheric influence and report any relevant findings in our final revised version of this paper.

2. Major Comment #2: The precision of the TOR retrievals should be better addressed. Perhaps a more detailed comparison be accomplished on shorter time scales and a discussion of these results be included.

Response: A similar question was raised by Stohl. The comment centers on the comparison between the TOR averages and the ozonesonde averages and whether or not shorter time periods could be used. In our Stohl response, we discussed some of the issues with using a smaller number of ozonesondes when trying to build a robust statistical relationship and also how the scope of this paper is geared more towards climatological and interannual relationships. We agree that shorter time periods instead of climatological time periods would provide better insight into how precise the TOR measurement is relative to the ozonesonde measurement. Some work has been done to look at the relationship between the TOR and ozonesondes over shorter time periods. Fishman and Balok (1999) looked at both daily relationships between the two in 1992 and monthly relationships between the two for 1988. Both showed very good agreement, in terms of correlation, bias and root mean square error. Also some additional comparison work is on-going that hopes to add to the body of work that is validating the use of the TOR.

3. Major Comment #3: A question was asked whether the latitude bands shown in Figure 6 could be extended to 55° N and whether we could add some reference to latitude bands in Figure 3.

ACPD

3, S1669–S1675, 2003

Response: In regards to Figure 6, the 50° N latitude band is the furthest north that the TOR data set goes. The reason is that the 50° N latitude appears to be the region where the climatological jet stream is located. In the winter, it tends to be at lower latitudes and in the summer tends to be at higher latitudes. But on average, the location is in general proximity to this latitude with the gradient of ozone surrounding these jet stream locations quite strong (Shapiro et al., 1982). According to Fishman et al. (1990), these gradients create the potential for a rapidly changing ozone distribution, especially during the winter and early spring. Based on these fluctuations in ozone, we feel that it is prudent to limit analysis in this area, especially during the winter and spring, to south of 45° N. In regards to the Figure 3 comment, we will add some reference to latitude bands in the final paper.

4. Major Comment #4: The comments by Parrish within the section "Major Specific Comments 4)" are broken down into four different sub-comments, which I will call 4.1, 4.2, 4.3, and 4.4.

Comment 4.1: A more detailed discussion of the significance of the correlation addressed in Section 4 should be considered.

Response: Our focus for this section was to discuss the relatively strong relationship between the springtime TOR over region 5 and the spring NAO index. We felt that the level of significance for this relationship was strong enough to discuss in terms of a climatological relationship. We understand, looking at Figure 7, that there are outliers in this relationship. However, after responding to a comment by Stohl regarding further analysis that could be accomplished between the NAO and TOR during this time period, the relationship between positive NAO/high TOR episodes appears stronger than negative NAO/low TOR episodes. In the revised paper, we will address the correlations and attempt to make the discussion more focused, including the recommendation about expanding the Region 5 area.

Comment 4.2: A clearer discussion of the 2nd paragraph of Section 4, the paragraph

ACPD

3, S1669–S1675, 2003

that discusses Figure 4a, should be included.

Response: We will take a look at this particular discussion and attempt to address the confusion surrounding the monthly progression of tropospheric ozone through the spring.

Comment 4.3: There is a 1994-1997 TOR data gap evident in Figure 7. Why are these data not available?

Response: The years of our study run from 1979-2000. However from 1993 to 1997, a combination of either TOMS or SBUV observations is not available. As discussed in Fishman et al. (2003), this is due to either no instrument on orbit or poor data quality. Therefore, the figure contains only those years for which both TOMS and SBUV are operational. For the final paper, we will add a note in the figure caption addressing this issue.

Comment 4.4: There is an unconvincing discussion centered on Table 2, could Table 2 possibly be eliminated.

Response: Taking another look at Table 2, we agree with the reviewer that this table is not value-added and the information that is being used from the table could be discussed in regards to Table 1. In the final version of the paper, we will eliminate this table and center the discussion on both Table 1 and the appropriate figures.

Minor Comments and Technical Corrections:

1. Minor Comment #1: A comment is made regarding the layout and size of Figures 2, 4, 5, and 8.

Response: As far as the figures, we are limited by the individual images that make up each figure. We will attempt, where possible, to rework the figures to make better use of the available space. Any revised figure will be part of the final paper.

2. Minor Comment #2: A comment is made regarding the great deal of detail in Figure

ACPD

3, S1669–S1675, 2003

1 and perhaps a simpler figure would be more appropriate.

Response: We agree that there is more information in the two images that make up Figure 1 than is being used in the paper. However, we thought it was a good representation of the location and relative strengths of the Icelandic Low and Bermuda/Azores that make up the two phases of the North Atlantic Oscillation, as well as the climate patterns and the strength of the westerlies that typically are seen during these two phases. We will add a comment in the figure caption stressing the points of the figure that are important for the reader to focus.

3. Minor Comment #3: A comment is made regarding the estimates of ozone flux from North America that we cite in our paper and how they appear to be on the low side.

Response: We will update our range of estimates of ozone flux to include the higher values shown in the cited paper (Li et al., 2002a). The revised numbers will be part of the final paper.

4. Minor Comment #4: A comment is made regarding the differing scales used in Figure 4.

Response: This same comment was made in the Stohl review. Here was our response: The two figures (4a and 4b) that comprise Figure 4 are each emphasizing something different. The focus for 4a is to highlight the differences in monthly climatological ozone between each region and we determined that the 25-50 axis range worked best for the range of ozone present within the regions. For 4b, we were looking to show the similarities between the ozonesonde profiles and the ozone derived from the TOR residual method and felt that the axis we chose best represented that relationship. Since each figure is emphasizing something different, we feel that our different choice of ranges for the axes is sufficient. Since the same comment was made by both reviewers, the scales appear to be a cause for concern. In the final revised version of the paper, we will separate out the two figures from Figure 4 (4a and 4b) and make them two individual figures (4 and 5).

ACPD

3, S1669–S1675, 2003

5. Minor Comment #5: A comment is made regarding the meaning of the last sentence of Section 3.3.

Response: We will review what was stated and, in the final paper, revise the wording to take out any reference to a delayed transport effect.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 4431, 2003.