

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.

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Response to Referee Comment S1314:

We appreciate and thank both the enthusiastic response and the insightful comments brought forth by Stohl of our paper. Our responses to the specific concerns, major points, and minor comments that Stohl discussed are addressed below. Responses to comments which require additional analysis or increased discussion will be reflected in the final version of our paper.

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Main concern: Stohl brings up a concern that uncertainties and possible artifacts from the TOR technique are not sufficiently well addressed and believes that a more thorough uncertainty analysis might be beneficial.

Response: During the interactive discussion phase of the publication of the TOR climatology in ACP (Fishman et al., 2003), much discussion centered on the actual technique, especially of the empirically-corrected stratospheric component. The results of these discussions were two appendices that were added to the final paper: Appendix A (Description of empirically corrected modified residual method) and Appendix B (Error analysis of empirical correction method). The uncertainties and possible artifacts of the technique were addressed in the paper and its appendices, and we feel that a more thorough uncertainty analysis, more comprehensive than the results shown in Fishman et al. (2003), is beyond the scope of this particular paper. However, there are efforts underway that are aimed at strengthening the validation of the TOR technique. One on-going study is directed at a comprehensive validation of the TOR technique against available long-running ozonesonde databases. The focus of this study is to identify the bias, root mean square error (RMSE), and correlations between long-running ozonesonde profiles and both the coincident uncorrected stratospheric component (SCO) and the empirically-corrected SCO. The aim is to determine whether this technique improves the uncorrected SCO and to what extent it captures the variability shown in the ozonesondes. Initial overall results show that there are strong correlations between the ozonesonde stations utilized and the coincident empirically-corrected SCO with a reduction in both the bias and RMSE. Similar analyses will be conducted comparing TOR values against the integrated amount of ozone in the troposphere from the available ozonesondes. These analyses are similar to the one performed in the Creilson et al. (2003) ACP paper (the coincident comparison of TOR against Hohenpeissenberg and Wallops Island ozonesondes), but expanded to look at a more global network of ozonesonde databases. It should be noted that several interpolation techniques are currently being examined to derive the best possible SCO from the relatively high frequency of SBUV profiles.

Major specific points:

Specific point #1: Stohl raises an issue regarding the retrieval technique and the validation of it utilizing available ozonesonde profiles. He believes that perhaps data for shorter time periods or using pixels directly at the ozonesonde location might prove beneficial.

Response: Stohl brings up an important subject, the validation of the TOR data. Regarding this paper, there are several issues with using ozonesonde data covering shorter time periods. The primary issue is that the number of available ozonesondes becomes much less than the amount of observations that go into a TOR data point. In order to build a statistically strong data set of ozonesondes to compare against, robust climatologies must be established. With shorter time periods, the confidence in the use of the data diminishes. A second issue centers on the scope of this paper, which focuses on monthly climatologies and interannual variability. We feel that the monthly ozonesonde climatologies provide the strongest method to validate the data. As far as using pixels directly over the ozonesonde location, we have looked at those data for this paper and have found similarly strong correlations between the ozonesonde profiles at the Wallops and Hohenpeissenberg locations and the actual TOR value within the pixel that is centered at that same location. We can put some discussion into the validation section of the final paper discussing these results. There have been other comparisons, which do cover shorter time periods, performed between the TOR and available ozonesondes (Fishman and Balok, 1999). This paper compared daily ozonesonde data against the daily SBUV data for select days in 1992, as well as monthly TOR against monthly ozonesonde profiles over Wallops Island for 1988. Good agreement was found between them. In our paper, we have cited the Fishman and Balok (1999) paper, as well as others, as references for validating the TOR against ozonesonde profiles.

Specific point #2: Stohl states that the authors should address whether the patterns we find in Figure 8 could be stratospheric versus tropospheric in nature (due to an artifact

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of the technique).

Response: The second paragraph in Section 5 generally discusses the subject of whether the ozone we are seeing in Figure 8 is tropospheric versus stratospheric. However, we agree that we do not specifically address a particular case when there is increased springtime ozone over this region and whether it may be stratospheric versus tropospheric. We have performed some potential vorticity analysis of an elevated springtime ozone case (1992) that was mentioned in the paper. We chose 1992 since it was an elevated springtime episode, the NAO was highly positive, and because we only have available PV data back to 1991. However we felt that both 1990 and 1992 (the two highest TOR years) were similar enough to perform the analysis. Simply stated, a review of the data, using the 2 PVU line as the demarcation between tropospheric and stratospheric air at the tropopause, shows that, especially for April and May, the ozone appears to be tropospheric in nature (over this region). We will add some discussion to this section in the final paper addressing this work.

Specific point #3: Stohl makes a comment about clouds and if they are possibly producing artifacts in the retrieval.

Response: The answer to this comment is in two parts. The first part will address the issue of clouds in the retrieval, while the second part addresses the relationship between the NAO and clouds. First part: We recognize that clouds can be an issue with the retrieval. The latest version of TOMS uses several corrections, including the International Satellite Cloud Climatology Project (ISCCP), to make adjustments for the amount of ozone in the presence of clouds and aerosols. Many studies have evaluated the performance of TOMS in this area (e.g., Newchurch et al., 2001; Liu et al., 2003). Newchurch et al. (2001) found that there is approximately a 4-9 DU excess of total column ozone over highly reflective cloudy regions. However, this paper also states that this excess will have a larger effect on the clear/cloudy difference techniques and less of an effect on the residual method when it comes to the derivation of tropospheric ozone from TOMS. There are and will continue to be some issues with the TOMS

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retrieval in the presence of clouds. However these and other studies have shown that the effects are minimized by the corrections to a point where meaningful and useful information about ozone can be extracted. Second part: We believe that during the positive phase of the springtime NAO, the Region 5 climate is in more of a subsiding air mass, indicative of warmer and drier conditions. We took a look at both the outgoing longwave radiation and the surface temperatures over Region 5 for a spring that has a very positive NAO and increased TOR (1992) and found highly positive anomalies for both. These conditions tend to be associated with less cloudy conditions, at least over this particular region (Region 5 in the paper). We agree that warm conveyor belt processes help to lift the air pollution up into the free troposphere and that they are associated with cloudier conditions, however this occurs primarily over the western part of the major oceans (at least climatologically). Thus, the pollution in the eastern U.S. would get caught up in the warm conveyor belt processes that are climatologically favored to occur in the eastern U.S. and get transported eastward in the westerlies. Once the pollution plume is moving eastward it may or may not be influenced by clouds. However, we believe that the increase in ozone we are seeing in Region 5 during the positive phase of the NAO is not occurring in cloudy conditions, but in the less cloudy eastern North Atlantic region. As a verification of less cloudy conditions over Region 5 during a strongly positive NAO spring (1990 or 1992), we will look at the reflectivity data from TOMS. This analysis will enable us to determine what the sky conditions were like over this region. Results of this analysis will be incorporated into the final paper.

Specific point #4: Stohl discusses a recent 15-year transport climatology by Eckhardt et al. (2003) that investigates the influence of the NAO on pollution outflow patterns from North America, and wonders if the patterns uncovered are consistent with what we found.

Response: We have not compared what we found in our paper with the transport climatology constructed by Eckhardt et al. (2003). However, it sounds very relevant to what we presented in our paper and agree it would be both interesting and worthwhile

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to at least compare the two and see what similarities and differences there are. In our final paper, we will address this comment.

Specific point #5: Stohl discusses the differences seen in ozone between the springs of 1980 and 1990, commenting specifically on the magnitude. He would like to see a composite analysis of low and high NAO springs either in addition to or instead of the highest and lowest TOR springs. He also thought that correlation maps between the NAO and ozone patterns might be a good way to help distinguish differences in ozone transport patterns, helping to better pinpoint mechanisms.

Response: We agree that compositing can be a useful analysis tool, especially when you have a large amount of data. Of the 18 years of eligible springtime data, there are 12 positive NAO springs with 5 strongly positive (NAO Index ≥ 1.0) and 6 negative NAO springs with 3 strongly negative (NAO Index ≤ -1.0). Due to the relatively small number (3) of strongly negative NAO springs during our study period, we were unsure whether compositing these events would provide meaningful results. There are a greater number (5) of strongly positive NAO springs and looking at Figure 7 it appears that the springtime relationship between a positive NAO and increased TOR in Region 5 is stronger for this relationship than for the low TOR/negative NAO relationship. We agree that compositing the 3-5 highest and lowest NAO springs might prove meaningful in determining if the increased TOR/positive NAO is more significant than the decreased TOR/negative NAO relationship. If we determine that the results are meaningful, we will add the resultant analysis to the final paper. We also agree that correlation maps might be an interesting look at the distribution of correlations in the springtime over that region. We plan on performing this additional analysis (to the highest and lowest NAO springs) and, again, if the analysis appears meaningful, include the results in the final paper.

Minor comments:

Minor Comment #1: A question is raised regarding the different axes ranges between

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Figure 4a and 4b.

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 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.

Minor Comment #2: A question is raised regarding the number of rows versus the number of years in the study shown in Table 2.

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. This is due to either no instrument on orbit or poor data quality. Therefore, the table has fewer rows than years in the study and 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.

Minor Comment #3: A comment is raised about possibly looking at a paper by Sprenger and Wernli (2003) regarding the influence of the NAO on stratosphere-troposphere exchange.

Response: We have now examined the referenced paper and found it to be very interesting. Unfortunately most of the paper focused on winter and summer where our focus ended up being on the spring. However we did find in the paper that during winters with a composite of positive NAO winters the stratospheric-tropospheric (STT) mass exchange stayed very far to the north, while during a composite of negative NAO winters the STT worked further south. The resultant correlations between STT and the positive phase of the NAO were therefore not significant over Region 5 of our study.

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These findings are consistent with what we found during some potential vorticity analysis of a positive spring NAO. We will continue to look for other research in this area which may be used for comparison.

References:

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