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Interactive Comment

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.

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General Comments:

The paper presents an important investigation of the transport of ozone over the North Atlantic Ocean. It is a very nice application of satellite data to tropospheric chemistry issues. My suggestions are for relatively minor revisions specified below.

Major Specific Comments:

1) Near the bottom of pg. 4433, the issue of anthropogenic vs. stratospheric influence as the cause of the spring ozone maximum seen at Bermuda is discussed. The impression is given that this is a question that has been settled with the conclusion that it is dominated by anthropogenic transport. However, I believe that it is still a subject of lively debate in the community. I think that the authors need to look through their analysis again to see if their data set can contribute to this debate, and to give a more balanced discussion of the issue. For example, in section 3 they discuss the late spring/early summer leveling and drop off of the ozone in regions 3 and 4. This is an even stronger feature of the Bermuda region. Is this not just what is expected if stratospheric input is driving much of this variability? The last paragraph in Section 5 argues that enhanced stratospheric input in positive NAO cycle is not the likely cause of the TOR/NAO correlation. However, this discussion should be expanded somewhat (although I agree that a detailed study is beyond the scope of this paper.) After all, the Icelandic low is not really a static feature, but rather at least partly the average of mid-latitude cyclones tracking across the North Atlantic. A deep Icelandic low implies stronger and more frequent cyclones. The dry air streams of cyclones represent tropospheric fold events that are responsible for transporting stratospheric influenced air into the troposphere. Hence one may expect a correlation between NAO and strat-trop exchange. Sprenger and Wernli, (J. Geophys. Res., 108, NO. D12, 8521, doi:10.1029/2002JD002636, 2003) discuss the effect of the NOA on strat-trop exchange, and this discussion should at least be referenced here. Also, Section 3.1 discusses the higher TOR over the North Atlantic than over the continents during non-summer seasons. They attribute this feature to slower photochemical production during those seasons. However, is it not also consistent with dominance of stratospheric influence in the non-summer months? The east coast of North America is believed to be a region of enhanced strat-trop exchange, which would yield a higher stratospheric influence in the troposphere downwind of the east coast, i.e. over the North Atlantic.

2) Section 3.4 discusses an impressive comparison between 22-year averages for each month for TOR and ozonesonde results. I concur with the review of A. Stohl

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that more detailed comparisons would be useful. Can it not be done on much shorter time scales? For example individual monthly averages to give about 250 points of comparison for each station, or sonde by sonde comparison. Either of these comparisons would give the reader a much better appreciation for the precision of the TOR retrievals. The monthly TOR averages are discussed in Section 4, but we do not have a clear idea of their precision. At least these issues should be discussed.

3) Can the latitude bands up to 55 N be included in the Figure 6 and discussion of Section 3.4? Then nearly all of Europe south of Scandinavia would be included. This region is perhaps of more interest from a photochemistry point of view than North Africa, which are the major land areas in the southern two bands. As it stands, much of their region 5 is outside the latitude bands that they include. Perhaps also, the latitude bands should be indicated in Figure 3 with dashed lines.

4) I have difficulty with much of the discussion in Section 4. First, in paragraph 1, my impression from Table 1 and the discussion is that the correlation between TOR and NAO is tenuous. Only two of the twenty correlations between season/region TOR average and NOA average are found to be "significant". The level of significance is not specified, but to have 2 out of 20 significant at the 95% level would not be too surprising, even if the data sets were completely uncorrelated. The most significant correlation is specified as "exceeding 0.01". If this means significant at the 99% level, even that is not too surprising for uncorrelated data sets, given that it is the only 1 out of 20 correlations that is significant at that level. Figure 7 does show a reasonable correlation, but it is for this one best case, and there are several obvious outliers from the correlation. Clearer discussion of the significance of the correlation is required. One possibility would be to expand the European receptor region 5 to a much larger area (say 35 to 55 N and 20W to 10E) with the idea that a wider average would better reflect the large scale circulation associated with the NOA. Second, in the second paragraph of Section 4, the discussion around the "monthly progression of tropospheric ozone through the spring exhibiting a greater difference in TOR from east to west during April and May than

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during March." is not clear to me. The difference is driven by the two source regions (1 and 5), with the other 3 regions showing remarkable similarity, even though they cover most of the east to west range. The difference between regions 1 and 5 may more reflect differences in photochemical environment or even latitude than longitude. And anyway it is not clear to me what this have to do with the correlation between TOR and NAO. Clearer discussion is required. Third, in Figure 7 the 1994-1997 data gap is disturbing. Why is it there? The NAO numbers are available. If the TOR data are not available, this should be discussed back in Section 3.1. Fourth, I find the discussion of Table 2 very unconvincing. If the seasonal correlation is tenuous, then further dividing the data into monthly averages (as in Table 2) will reduce the significance much more. Further to identify a year long period as high NAO is a difficult proposition, since the NAO Index seems to have a great deal of variability, often switching signs on a month to month basis. Perhaps Table 2 should be eliminated and the discussion of low summer correlation carried out with regard to Table 1.

Minor Comments and Technical Corrections:

1) Figures 2, 4, 5 and 8 have small panels with relatively large amounts of blank space between them. I suggest that the panels be enlarged to eliminate that space, and that where possible symbol definitions, color keys, and other labels be included within the figure panels.

2) Figure 1 seems to have a great deal of detail, which is difficult to understand from the information given in the caption. Perhaps a simpler diagram would be more appropriate.

3) In the first paragraph on pg. 4434, the literature estimates of ozone flux from North America are discussed. However, the numbers given are at the low end of the range of model results. Section 5 of their reference Li et al., 2002a discuss some estimates and arrive at values near 6 Gmol/day. The lower values cited in the present paper are for Eastern North America only.

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4) In Figure 4 it would be useful to use the same scale in both panels, say 20 to 55 DU.

5) In the last sentence of Section 3.3 the authors seem to suggest that the later peak in region 5 might be due to a delayed transport effect. Really the transport times across the Atlantic are too short for this to have a noticeable impact on a monthly scale.

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