

***Interactive comment on* “Surface ozone depletion episodes in the Arctic and Antarctic from historical ozonesonde records” by D. W. Tarasick and J. W. Bottenheim**

Anonymous Referee #2

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Review of "Surface ozone depletion episodes in the Arctic and Antarctic from historical ozonesonde records", by Tarasick and Bottenheim, submitted to Atmospheric Chemistry and Physics.

This manuscript presents a summary of the number of occurrences of observations of ozone mixing ratios less than approximately 15 ppb near the surface from ozonesonde measurements from 7 Arctic stations and 3 Antarctic stations. This short paper is well written and the results may be quite interesting to those concerned with surface ozone chemistry, there is however, not a lot of detail beyond the presentation of the occurrence frequency as a function of season. In particular there is a disconnect between the authors' statements in the abstract and conclusions, and those in the main body of the

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text. The statements in the abstract and conclusions are a bit misleading and should be changed to reflect more correctly the analysis.

The statements referred to concern the extent to which the observed ozone minimum at the lowest levels can be attributed to surface meteorology. In both the abstract and conclusions statements to this effect are found, particularly in the abstract. Yet in the main body of the text, bottom of page 6 top of page 7, it is stated that none of the meteorological parameters examined, stability, winds, surface warming, showed any correlation with the frequency of depletion events. In fact the authors conclude, page 7, that other factors play a more dominant role. Thus the statement in the abstract is wrong and that in the conclusions misleading, although the latter is qualified immediately to state that the supposition is not supported.

Other suggestions for improvement.

1) Table 1. Include the station latitude, and its distance from open water. Include the results from Syowa even though no depletion episodes were found. Make an objective assessment of whether it is important to separate out the Brewer-Mast results. If it is not important, i.e. if the results are not significantly different if the BM and ECC sondes are separated or not, then I would be inclined to include all measurements together.

2) Page 4, line 19, "As anticipated \checkmark " Why were these results anticipated?

3) Page 6, top of page. The outliers 1989-1991 were removed, but what about those in 1994. Are these not outliers because similar frequencies were observed at three stations. What would cause this high degree of occurrence and correlation in 1994?

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 339, 2002.

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