Atmos. Chem. Phys. Discuss., 6, S672–S673, 2006
www.atmos-chem-phys.org/acpd/6/S672/
European Geosciences Union

ACPD

6, S672–S673, 2006

Interactive Comment

Interactive comment on “The influence of polar vortex ozone depletion on NH mid-latitude ozone trends in spring” *by* S. B. Andersen and B. M. Knudsen

Anonymous Referee #1

Received and published: 27 April 2006

The paper is amending findings from an earlier study published by the two authors in Nature. Even though uncertainties in this study are large the authors attempt to duly discuss the problems of their approach. Nevertheless some aspects of their methodology require further discussion before the paper should be published in ACP.

1) Why are the ozone depletions taken from different sources? Homogeneity in the methodology of determining ozone depletion seems mandatory to me.

2) The regridding approach requires further explanation. I couldn't quite understand how it was done in this study.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

3) Building on the previous point, I feel that an earlier discussion of the uncertainties related to the chosen methodology would be helpful.

4) Coming back to my point made in the access review: I guess it would be extremely helpful if the authors could be convinced to use only one type of map projection (no preference on my side), so that the reader can appreciate figures 1, 3, 4 and 5 fully.

5) Even though I agree that longitudinal differences in trend are very important and that RDF is an appropriate tool to use to look at those, I am feeling slightly uneasy about the numbers presented in the conclusion. Again I would request a stronger emphasis on the uncertainties when summarising the results. People tend to read abstracts and summaries/conclusions; I am slightly worried that the message provided by the conclusion as it stands is not the messages that should be taken away from this paper. I would request a stronger emphasis on the spatial distribution and inter-annual variability and less emphasis an eye catching absolute numbers (the same is true for the abstract).

6) On page 1800, line 25: I don't understand the sentence - the authors have 5 years and the EESC is not very well defined in its temporal development. What are the authors trying to say? That additional ozone depletion may influence their results? I believe this is true anyway, but how does this relate to a mean dilution of 9.8 with a substantial error bar?

In summary I would request a much more thorough discussion of uncertainties in this methodology and a stronger emphasise on the spatial patterns before the paper is accepted for publication in ACP(D).

Being late with my review I had the advantage of already reading the assessment by reviewer 3 and can only support the different/additional points he is making.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1793, 2006.