

## ***Interactive comment on “On the attribution of stratospheric ozone and temperature changes to changes in ozone-depleting substances and well-mixed greenhouse gases” by T. G. Shepherd and A. I. Jonsson***

**T. G. Shepherd and A. I. Jonsson**

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We would like to thank reviewer 3 for his/her kind words and useful comments. Below we address his/her concerns.

1. Question: "The authors state clearly that they ignore the role of heterogeneous chemistry. It is not clear to me why that is necessary. Wouldn't that be part of the ODS contribution anyways? I realize that they may end up using a different proxy for ODS, but again, why not?"

Answer: This statement (Section 2, line 15, page 4) was intended for the analysis in

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Section 2 of the paper, in which we linearize the ozone and temperature relationships. Polar ozone loss through heterogeneous chemistry is a highly non-linear process (due to the step like temperature sensitivity of chlorine activation, and the multiplicative sensitivity of temperature and chlorine). Thus, a linear approach, such as the method presented, will fail to correctly represent the ozone temperature relationship in polar ozone loss regions. (Also, the sign of the temperature dependence would be opposite to that assumed here.) However, for the attribution analysis in Section 3 the reviewer is in principle right. All effects, including e.g. CH<sub>4</sub> and H<sub>2</sub>O trends as well as ozone loss through heterogeneous processing, will affect the attribution analysis. While not included as a separate attribution term, heterogeneous ozone loss would be implicitly included in the CFC attribution term (while H<sub>2</sub>O effects would primarily be implicitly buried in the CO<sub>2</sub> term). This will be made more clear in the revised version of the paper.

2. Question: "Is solar variability included in the model runs that are used for the attribution? The period examined for the past includes about two solar cycles: if solar variability is included, how is that factored in the ozone figures (fig. 4a)? Or is the assumption that the average of such a period cancels out any effect of the solar variability? How dependent are these results on suggested changes (from observations) in the solar constant that models typically use?"

Answer: Solar variability was not included in the simulations used for this study.

3. Question: "Figure 4. The x-axis is labeled "Mixing ratio [%/decade]" It is not clear if the units are pure mixing ratios (as in ppmv) or in %/decade."

Answer: Figure 4 shows linear trends in ozone mixing ratio expressed in percent per decade. The label "Mixing ratio" was added to clarify that it is the trend in "mixing ratio" (as opposed to the trend in "number density", which theoretically could be slightly different) that is shown. To avoid confusion the x-axis labels of Figs. 4 and 5 will be changed to "[%/decade]" and "[K/decade]", respectively, in the revised version of the

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paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12327, 2007.

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