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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Extrapolating future Arctic ozone losses" *by* B. M. Knudsen et al.

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In the interest of stimulating the discussion we here reply to the two comments by John Austin published on 1 July, 2004. Referee 1 has raised some quite relevant points in the review, which we will address later. We have only responded to the most important points made in the comments by John Austin, but all point will be considered when revising the paper.

It is true that inferring future PSC changes from past changes is dangerous. However, since the understanding of the past trends is poor, our approach is to consult nature and ask what has been going on for the last 43 years. There are bound to be large errors in this approach, as discussed in the paper, but can we be sure that these errors are larger than the errors involved in CCM modelling? Our calculations cannot replace the CCM models, and it is very important to carry on developing the models, but we think our calculations gives a prediction of the future which cannot currently

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be dismissed. The future development of the planetary wave forcing is unknown, and there is no consensus about it in the CCM's.

Austin suggests that the chaotic nature of the atmosphere may be the cause of the differences between the CCM's. We think this is an unlikely explanation, since each of the models should be able to catch most of the chaotic behaviour of the atmosphere. The interannual variations in modelled ozone does not seem able to explain the huge differences between the models. Anyway, the models all have some common biases as for example the neglect of denitrification, which will force the model results towards too little ozone depletion.

John Austin suggests that the water vapour trend should be set to zero. This is an unlikely scenario since methane is thought to increase in the future. It still remains to be seen whether the decrease in the last few years is a temporary drop (of which there have been a few in the Boulder series) or a more permanent change. The Joshi and Shine (2003) study suggests that volcanic eruptions may explain 1/4 of the observed water vapour trend since 1955 for an e-folding time of water vapour of 5 years and much less since 1963. We agree that the water vapour trend is uncertain, but we think a trend of 1% per year is more likely than no trend at all based on the evidence at hand. In the paper several sensitivity test regarding this are described.

John Austin rightfully questions whether midlatitude trends in water vapour can be used in the Arctic. To our knowledge no trend estimates for the Arctic water vapour exists, so we will add a word of caution. In the paper sensitivity studies show the effect if there has been no trend at all in water vapour in the past.

John Austin points out that there is a considerable cancellation of errors in the CCM's. This is a valid point, which we will add in the revised version of the paper. The cold pole bias cannot completely balance too small amounts of chlorine, however, but would probably lead to too small depletions in cold winters and too much depletion in warm winters.

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