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Interactive Comment

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

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**Is there really an alternative to the use of coupled chemistry-climate models? Part 3**

## General Remarks

In their response to my previous comments, Part 1 and Part 2, Knudsen et al. indicate more confusion concerning the performance and characteristics of chemistry climate models (CCMs). While they exaggerate the problems of CCMs, I believe they continue to downplay the errors in the interpretation of observations and their extrapolation well into the future. Whenever there is a weakness in the observations the authors seem to favour data or explanations which support slow ozone recovery (high H2O trends, not using recent data, high temperature trends, neglecting the impact of ozone in contributing to those trends). Unless this is remedied in the final version, the paper will in

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my view appear unbalanced and alarmist. I am certainly pleased to hear that Knudsen et al. consult nature. However, some of us arrive at different answers when we do so.

**CCMs**

It is easily verifiable that most Chemistry Climate Models nowadays include denitrification in some form or another, yet in their last response Knudsen et al. state boldly: ‘the models all have some common biases as for example the neglect of denitrification’. Modellers generally work very hard to include all relevant processes (and in my biassed view succeed!). Of the Austin et al. (2003) paper, results from 8 models are included. In the model description pages, 5 of the models state explicitly that sedimentation is included, one model explicitly does not, one does not specify and the final model has parameterised chemistry so may be safely assumed not to have denitrification. Therefore I would not say that this is a common bias! I would say that the only process that is missing from most of the models is the impact of volcanic eruptions, but the significance of this is now starting to become clear, long after our simulations are complete (more about this later in this response).

Regarding the future behaviour of planetary wave forcing, perhaps my explanation was unclear but perhaps a better example of what I was trying to communicate is recorded in WMO (2003), pages 3.66-3.67. Two of the authors of Knudsen et al. (BK and MR) are also coauthors of that report but I am not aware that they offered any dissent before publication. Perhaps the authors might also refer to Austin et al. (2003), Figure 9. This is of course not the diagnostic that might be of prime interest to the authors, but it does suggest rather moderate model interannual variability. Thus it is not inconceivable that the real atmosphere if it were to follow any one of the models, might give any result from full ozone recovery by 2030 to almost no change in ozone amounts. Of course the models are biassed high relative to observations but we are all working to improve that. For example, UMETRAC appears has a high bias in tropospheric ozone.



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Knudsen et al. raise again the issue of cold biases. As I explained in my previous comment not all models have a cold bias in the lower stratospheric Arctic as tropospheric impacts are also important. For example, in Austin et al. (2003), Figure 3, all but one model has a warm bias at 50 hPa during the winter and during the spring it is more evenly divided, 5 do indeed have a cold bias of up to 6K and 3 have a warm bias of just a few K. However, for those models that do have a cold bias. As Knudsen et al. suggest interannual variability might be negatively affected. However, some models seem to give excessive interannual variability in the South (CMAM, UMETRAC) but others seem satisfactory. Most models also seem satisfactory in the North according to the diagnostics presented in our comparison paper. The relationship is not as clear in a CCM as a CTM for the reasons noted in the previous remarks, that there are compensatory errors built in to the systems.

I did not understand their response to my comment about transport impacts, in particular their sentence 'However, transport affects also the present ozone, so this could not explain why many models show substantial recovery by 2030 contrary to our results.' My argument is that we are working with a coupled system and as the chemical changes occur so they will induce temperature and planetary wave changes which will induce transport changes. Thus at the ozone minimum, corresponding to c.2000 or so, transport and temperatures were also at a minimum. From 2000 onwards, if and when we see ozone recovery, then both temperatures and transport will increase together. Hence, the ozone increases much faster than the reduction in ozone depletion alone and of course during the depletion phase in the past presumably ozone has decreased faster than the chemical depletion alone. Hence those models which show a future temperature increase will also show a reduction in depletion and an increase in ozone transport to first order. Of course because it is a coupled system it is not appropriate to think of the temperature driving the ozone or the other way around — they work together. In some models, there may also be increased transport due to possible increases in tropospheric planetary waves. Similarly, if the author's projections turn out to be correct, the change in transport would also be small and presumably lead to an overall small change in ozone. I am assuming that by recovery, the authors mean a significant increase in column ozone.

**Temperature change in the lower stratosphere**

There are of course many problems associated with attempting to calculate past temperature trends in a region such as the Arctic where few radiosondes presumably exist. The differences quoted by the authors in using different data sources only highlights this. Just because two of the three happen to agree, and only for two particular winters, does not in itself justify their use to the exclusion of all others. A more comprehensive and less misleading approach would be to use the other observations as well and to bring the results up to date. I accept that the updated trends might be within the error bars, but since the errors are quite large it could still make a substantial difference to the ozone projections as far as 25 years away. I am disappointed that the authors dismiss my zero temperature trend projection as unlikely. This is a rough generalisation from several climate models. So, many people might think that this is the best-guess number. Incidentally, the Randel and Wu (1999) paper indicates that temperatures over the Arctic between 17 and 18 km have already dropped about 8K during March, so if this is extrapolated in the same spirit that Knudsen et al. have done, we would end up by 2030 with a further 12K loss. This is approximately what is needed to achieve the PSC extrapolation of Knudsen et al. So, a cold arctic winter would become Antarctic-like which I don't think is likely on dynamical principles (too much wave forcing from land-sea contrast). I am of course fully aware that my request, disappointingly rejected by the authors, for a curve showing the impact of zero trends would follow the EESC line. It would in any case help to put the others into context. One could also remove several of the lines from Figure 5 which portray an unrealistic indication of uncertainties.

**Water vapour trends and volcanoes**

I do not want to make a big issue of this as the most important issues affecting ozone



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are the temperature trends and transport changes. Nonetheless, in the interests of completeness I shall expand on my previous remarks. Water vapour is indeed influenced by $CH_4$ oxidation but the tropical tropopause temperature has also been decreasing slowly at about 0.5K/decade (Seidel et al., 2001) and because of the substantial temperature dependence of the saturated vapour pressure, the two terms approximately balance for the past. Since ozone trends in that region are relatively small, it might be anticipated that the tropopause trend is primarily due to trends in WMGHGs and that this trend will continue into the future.

There are several ways of interpreting the results of Joshi and Shine (2003). Knudsen et al. take the lower limit suggested in the text of that paper of 0.25you can look at their Figure 9 part (c), which suggests possible increases between 1962 and 1992 of about 37%, larger than that observed during the same period. Also, Mastenbrook and Oltmans (1983) comment on the reduction in water vapour over Boulder during the middle to late 1970s qualitatively similar to the Joshi and Shine Figure 9. As I noted last time, this leads to a discontinuity in the trend lines for the two sites. Joshi and Shine accept that their calculations are simplified but with some improved asumptions they might fit the observations better in terms of perhaps the timing of the peak water vapour concentration. Personally, I look forward to their more detailed study.

**Summary**

The authors have consistently argued that their trend projection showing little ozone recovery is a ‘most likely scenario’ whereas I would continue to argue that it is more like an upper limit. This follows from their apparent tendency to reject data which do not fit their preconceived ideas and the questionable procedure of extrapolating high trends 30 years into the future. This will require the Arctic to appear dynamically similar to the Antarctic. We know that the atmospheric conditions have changed because of the regulations on CFCs, but this is only taken into consideration in the direct chemical effect. Therefore many of the trends will change substantially over the next few decades. The authors comments and paper are contradictory in apparently accepting



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the dangers but not providing alternative calculations if their assumptions happen to be incorrect.

**Additional references**


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