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Interactive Comment

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

J. Austin

john.austin@noaa.gov

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Regarding temperature trends, there are data available from other sources than Berlin. The fact that the authors are not prepared to use these data is worrying. In any case computing trends whatever the sources over such long time periods is a problem as indicated in my comment to Gianni's criticism. Whatever the details of recent years, Knudsen et al. cannot be certain that the recent temperatures are not an indication of the new regime caused by ozone stabilisation. As I have indicated the authors have *not* taken into consideration ozone trends because only a small fraction of the observed temperature trend is likely to be the radiative effect of GHGs. Without a model looking at the processes in detail, forward extrapolations may not get the order of magnitude of the temperature trend right or even its sign.

The GFDL model is typical of many models and has a peak age of air of 5.0 years at the pole during spring at 30 hPa. If the authors know of data at this location I would like to know as I have been trying to obtain such data. However the only data I could find, which peak at an age of just over 6 years, support the argument that the GFDL model underpredicts age by one third consistently. If this were maintained within the Arctic vortex it would suggest that the age of air peaks at about 7.5 years there. In my previous comment I took the devil's advocate value of ten years. We know it's at least five years and who is to say that in unobserved regions the value is not as much as ten, although 7-8 seems more likely. The volcanic issue has not been thoroughly explored. Knudsen et al. reject it at their peril.

I don't really understand the modelling criticisms. Earlier, the problem with CCMs was that we didn't have denitrification. Now, when it is admitted that we have it, the process must be incorrect because not enough is known about the atmosphere! If that were the case, can we now conclude that we don't know enough to decide whether it should be included or not? If we don't know enough, won't the situation with the hypothesised increase in water and PSCs make the Knudsen et al. extrapolations of the past more susceptible to error? Incidentally, all the schemes are based on physical principles. The question is whether such principles apply in the atmosphere and to what level of detail.

I am surprised to find yet another weakness of our models: the well-mixed GHG scenarios are apparently 'uncertain guesses'. Actually, I think these are well established out to 2030, although they could be subject to political influence. The major assumptions are CO_2 for radiative forcing and CFC and halons primarily for halogen amounts. The Knudsen et al. paper uses essentially the same CFCs and halons. Extrapolation of past temperature trends as in Knudsen et al. implicitly assumes continued increase in CO_2 or ozone depletion, or dynamical change at about the current rate. (If it were primarily due to ozone depletion this would set up a logical inconsistency with the Knudsen et al. paper as indicated in my comment to Gianni). So, unwittingly, perhaps, Knudsen et al. has the same basic problems lurking in the background as CCMs, only worse. The lack of recognition of this only serves to convince me that their errors are larger than stated. Now have I provided convincing arguments that *there is currently no alternative to the use of coupled chemistry climate models for predicting future ozone amounts*?

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