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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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General comments:

I believe that this is a novel approach to predicting future Arctic ozone trends, which merits consideration by the community alongside CCMs given the uncertainty in the latter models. Though I'm in general wary of extrapolating trends, it appears to me that the authors have done a credible job of basing their extrapolations on the physical principles thought to underlie the past changes. The cause of the water vapor trend, however, remains quite uncertain, so I believe it would be useful to further clarify how dependent upon that trend are the results. The authors have done this in most of their calculations, but I suggest an additional quantification below (section 3 comments). I believe this paper could benefit from a few additional relatively minor clarifications, especially as to the dependence upon the resolution of the temperature data (section 2 comments). Overall, though, the paper is well-written, presents novel and apparently well-reasoned results, and makes a useful contribution to the literature.

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Specific comments:

Abstract

First sentence: It would probably be better to say that future greenhouse gas and water increases 'may' cool the stratosphere, rather than 'are likely to', given the current uncertainties.

Section 1

Page 3229, line 6: The authors should add a reference to the Oinas et al., GRL, 2001 study along with the Forster and Shine work cited here, as the studies give somewhat different quantitative results though they are qualitatively similar.

Section 2

The analysis of the temperature data was done on 10 degree grids. While the authors note that calculating the PSC areas at higher resolution had only a minor effect, I'm surprised at this result. The paper would be improved by adding a quantification of how the PSC areas change using the higher resolution ECMWF data. That data goes up to T159, and unless the field is extremely smooth it seems odd that the resolution would have such little effect. If true, can the authors account for this? What higher resolution did they try? Along the same lines, I believe that a more thorough explanation of how the predicted area of temperatures below the NAT condensation point on 10 degree grids were evaluated against radiosonde data from single points would be useful. I don't understand what the percentage differences given mean.

Section 3

The second paragraph begins by describing the dashed black line giving the PSC trend without a water trend. The authors should state if this PSC trend is statistically significant (i.e., does the trend become significant only with the presumed water trend included?). The next sentence describes the solid red line. It would be helpful to state that this includes the water trend (I presume) since it immediately follows the sentence

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about leaving out the water trend.

Section 4

The authors should give the years included in the Rex et al calculation of vortex averaged ozone loss (first sentence).

Section 6

Both WMO (2003) and Austin et al (2003) show comparisons of minimum Arctic total ozone column simulated in the various models. The current paper, however, calculates total vortex ozone depletion. There is no guarantee that the CCM trends for the most extreme point in their distribution of simulated Arctic ozone values will bear any resemblance to the total vortex depletion trends simulated in those same models. Thus a quantitative comparison between the present results and the CCMs is not appropriate, since they are not the same quantities. Of course, for those models that showed weaker ozone depletion than the authors' results, that difference will still be true given that you are comparing with the CCMs maximum value. However, for the opposite case, e.g. the GISS model, for which the authors state that it 'showed much more ozone depletion than our results by 2010-2020', the authors should be more cautious, and note that this is the trend in the maximum loss at a single point in the CCM compared with the total vortex amount. Similarly for the 2030 comparisons with CCMs, the model results are the maximum ozone depletion, which is indeed reduced in most CCMs. However, that does not necessarily imply that the total loss is reduced. I have not seen the total vortex losses published for all those models, so the authors may not be able to make a more appropriate comparison.

Figures

I had a hard time distinguishing the squares on Figure 1. Perhaps the large could be a different color than the small ones, and both could be bigger.

Technical comments:

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Page 3230, line 21: There is an extra 'the' before 'both the ERA-40'.

Page 3236, line 13: The word 'thought' should be replaced with 'expected'.

Page 3238, line 4: The word 'predicts' should be singular.

Page 3238, line 15: The words 'underestimates' and 'does' should be singular.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 3227, 2004.