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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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Is there really an alternative to the use of coupled chemistry-climate models? Part 1

General Remarks

The paper by Knudsen et al. attempts to predict future ozone losses using a variety of techniques based on observations of the current atmosphere. It provides in many cases quite valid criticisms of coupled chemistry climate model (CCM) results. However, the paper lacks balance in the interpretion of those results but instead supplies an extrapolation technique which in my view may be quite misleading. Predictions of future ozone are strongly dependent on future temperatures which their technique cannot provide. Hence I entirely refute the suggestion that their method provides an alternative to the use of CCMs. I believe that the correct way forward is to continue to

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develop CCMs to provide improved simulations and then to use the type of techniques developed by the authors to help diagnose model performance.

Temperature change in the lower stratosphere

The abstract is misleading. The radiative impact of increases in greenhouse gases, or strictly the well-mixed greenhouse gases (WMGHGs) and water vapour do indeed dominate in the global average to give a cooling (e.g. Shine et al., 2003, Ramaswamy et al., 2001). Over the Arctic the future impact of the WMGHGs on the lower stratosphere is uncertain at present because of their influence on the planetary wave forcing. This is critical since the increase in Polar Stratospheric Clouds (PSCs) in the authors' extrapolations supposes continued further cooling of the stratosphere. As noted by the current authors the Arctic is in a 'delicate balance between competing processes' and determining trends therefore requires the very models the authors eschew.

In many cases, planetary wave dynamics plays a significant role, and some models indicate a future cooling of the lower stratospheric Arctic, (e.g. Shindell et al, 1998, Austin and Butchart, 2003), while other models indicate a future warming of the lower Arctic stratosphere (e.g. Schnadt et al., 2002; Butchart et al., 2000 — no ozone trend). Of course one of the possible causes of these differences is that the arctic atmosphere itself may not be predictable to that level of detail. Indeed, the changes in the models are in many cases not statistically significant. Thus we may only be able to simulate with different models the range of plausible scenarios. This was a point raised in WMO (2003) Chapter 3, in which it was suggested that because essentially of the chaotic nature of the system, even a perfect model may be incapable of predicting the precise future behaviour of the Arctic. The authors' extrapolations should also be subject to the same chaotic influences, but at least with CCMs a range of solutions is at our disposal from which we might get a better indication of the predictability of the system.

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It is unarguable that the lower stratosphere has cooled in the past (Ramaswamy et al., 2001). There are some questions over the extent of this cooling at least as far as the PSC areas are concerned. Incidentally, why do we not see the latest 3 years of results in Figure 1 and the latest 4 years in Figure 2? Adding these points to Figure 1 could have a substantial impact on the computed trends. In Austin et al. (2003) the PSC areas were computed from NCEP data using fixed H_2O and HNO_3 and these showed a much smaller trend than the results shown here. Knudsen et al. have extrapolated the H_2O trends backwards well before the Boulder data series (Oltmans et al., 2000). In that paper, the water vapour trend in each of the Washington, D.C. and Boulder datasets indicate an upward trend of order 0.05 ppmv/year as assumed by Knudsen et al., but the datasets do not appear to be entirely consistent with each other despite being midlatitude sites. For example, in the DC dataset, the values of the regression line through the data give a value of about 4 ppmv in 1965 compared with the 2.85 ppmv assumed by Knudsen et al. and the 3.5 to 4 ppmv in the Boulder data in 1980, depending on altitude. Thus, if the DC and Boulder data were merged into a single dataset, it is possible that the trend over 35 years would be about 0.03 ppmv/year, some 40% smaller than that assumed. Randel et al. (2004) also indicate that the past water vapour increase may have recently reversed. For example, in the Boulder data series during the period 2000-2002, the Boulder trend was down and by the end of the dataset the values were again about 4 ppmv in the altitude range 17-22 km. Thus one might argue that the water vapour change for the period 1965-2003 is close to zero, consistent with HALOE data for 1992-2003 and vindicating climate model simulations. These changes in water vapour trends may be related to volcanic activity (Joshi and Shine, 2003) so by assuming a continued increase in water vapour at the rate that they indicate, Knudsen et al. may be implicitly assuming an increase in volcanic activity.

The main issue is whether past temperature trends are an indication of future trends.

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The current authors quote GHGs (I assume WMGHGs) and water vapour as the main factors in the determination of stratospheric temperatures. Randel and Wu (1999) suggest that it is the ozone itself which has caused the cooling. Although strictly they only show correlations which is not the same as implying a causal mechanism, the spring Arctic cooling is about an order of magnitude larger than that associated with WMGHG increase. Further, what is the evidence for any water vapour change in the high latitudes of relevance to PSCs? As we have seen with the above argument, observations are of limited scope and tend to be concentrated in the middle latitudes (SPARC, 2000). Unless the physical mechanism for a water vapour increase is identified unambiguously, extrapolating selective recent trends up to 30 years into the future may be a much worse assumption than relying on CCM predictions. The only thing that seems to be definite is the increase in CO2, but a massive radiative perturbation is likely needed to cause significant further cooling and further ozone depletion with fixed chlorine (Austin et al., 1992). Clearly we have a potential difficulty in separating cause and effect, but is it not feasible that the past ozone trend was primarily due to halogen increase and that this triggered the Arctic temperature trends? If so, it would follow that the future temperature trends will be only a small fraction of the past trends. While acknowledging that there is difficulty in the causal attribution of past temperature trends (WMO, 2003, Chapter 3), the authors carry on regardless with their own implicit assumptions.

CCMs and CTMs

The criticisms of CCMs are largely valid when taken in isolation. Many of these points are reasonable extrapolations from what has been learnt with CTMs but this fails to consider the compensating impacts of the different issues. In CTMs the temperatures and winds are explicitly specified and it follows that temperature dependence of the physical processes in the model will affect ozone directly. In particular, ozone amounts

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will be sensitive to the PSC scheme. In contrast the performance of CCMs are not that sensitive to the PSC scheme for the simple reason that in most models the parameters are adjusted so that agreement with current ozone observations is achieved. For example, models with a cold bias may use a thermodynamic barrier (e.g. Hein et al., 2001). Those with a warm bias may get better results with ternary solutions (e.g. Austin and Butchart, 2003). The authors also mention that models underpredict chlorine but in the presence of too many PSCs, the ozone depletion rates may still be qualitatively correct. It follows from this that such imperfect models may be unreliable as trend indicators. I agree, but conceptually the best CCMs may have now reached a similar level of performance as the climate models of a few years ago. Then, for example, flux adjustments (IPCC, 2001, Table 8.1) were needed in many models to simulate an accurate current climate. Such models give useful climate predictions and similarly we continue to get value from CCMs. A number of the other issues (NAT rocks, Fahey et al., 2001; sedimentation, Waibel et al., 1999 etc.) are significant only once the temperatures are correct.

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