

## ***Interactive comment on “Montreal Protocol benefits simulated with CCM SOCOL” by T. Egorova et al.***

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The authors present a comparison of two simulations using the CCM SOCOL, one with stratospheric halogens as projected under a Montreal-Protocol scenario, and one assuming unrestricted exponential growth of halogens throughout the length of the simulation. They find very substantial differences in total ozone, and concomitant climate change. They conclude that the MP has been of considerable benefit to protecting the ozone layer and climate. The results are not fundamentally new, as acknowledged by the authors. Similar results have been presented before by Morgenstern et al. (2008) and Newman et al. (2009). Most of the results presented here are consistent with these earlier studies; quantitative differences are within the range usual encountered in inter-model comparisons. WMO (2011) points out that certain model limitations may

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impact these earlier results, namely inadequate tropospheric chemistry, off-line photolysis that prevents the ozone depletion from adequately affecting chemistry, and the absence of an interactive ocean. This latter deficit means that surface climate change may be unrealistic as the ocean does not respond to the changed radiative forcing. The authors claim that their model is more comprehensive than Newman et al. and therefore their results are more credible. Tropospheric chemistry, to my understanding, is not particularly comprehensive in SOCOL (this needs to be spelled out explicitly); this might impact the realism of their simulation in the troposphere. Their photolysis scheme would need to be online (i.e., an explicit calculation of actinic fluxes, taking into account absorbers and scatterers) to be better than these earlier results. This is also not spelled out explicitly.

Answer: In the GEOSCCM used by Newman et al. (2009) tropospheric ozone is prescribed, and relaxed to the Logan (1999) climatology. It means that GEOSCCM does not have tropospheric chemistry at all and tropospheric ozone is not responsive to the introduced perturbations. Morgenstern et al. (2008) used fixed photolysis rates in the troposphere that means that tropospheric ozone does not react to UV changes caused by stratospheric ozone depletion. We do not claim that our results are more credible because it depends on the magnitude of the tropospheric ozone changes, but we think that our paper is definitely a step forward in this direction because the applied CCM SOCOL includes basic (w/o VOCs) tropospheric chemistry and on-line calculations of the photolysis rates. To clarify these points for readers, we have extended the Introduction emphasizing the difference between the CCM SOCOL and previously used models.

They do not include an interactive ocean. This is of particular relevance to their surface temperature and precipitation changes in response to ozone depletion that show a lot of geographical detail. Such detail is known to be model dependent; the credibility of this result is further undermined by the non-interactive ocean which causes an almost zero temperature difference between the two simulations over sea. I sug-

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gest that instead of studying the surface temperature and precipitation directly, which does not make much sense in view of the uncoupled nature of their model, the authors could study the behaviours of the Northern and Southern Annular Modes (NAM and SAM). These modes are presumably influenced by the ozone depletion, are deep features connecting the stratosphere and the troposphere, and have known surface temperature expressions. So by studying the difference in NAM and SAM signatures around the tropopause level or higher between the two simulations, an inference could be made about how this would translate into a surface temperature difference and possibly a precipitation difference in a fully coupled model. The substantial cooling over Siberia under the NMP scenario is consistent with a strengthening of the NAM which would likely be found by this analysis. In order to account for the substantial climate change due to increasing long-lived greenhouse gases found in both simulations, the EOF analysis could be done just on the difference between the two model simulations, cancelling out this influence.

Answer: It should be noted that the NAM/SAM behavior is also sensitive to the absence of interactive ocean and its response to the introduced forcing is also model dependent. Showing some quantities characterizing surface climate we just would like to emphasize that the Montreal Protocol limitations can have implications not only for the ozone layer but also for climate. It was also mentioned by Morgenstern et al. (2008), however the lack of strong radiative forcing from ODS in their model leads to an underestimation of the climate effects. This new aspect of the problem can be adequately addressed only with CCM coupled to interactive ocean. Anyway, to fulfill the reviewer's request we have added the analysis of NAM and SAM signatures.

Details:

P17003I27: Replace "present-day" with "year-2000". Since then, the chlorine loading has dropped below 3.5 ppbv. Answer: done

P17004L10: "a threefold increase" Answer: done

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P17004L16: “the absence of realistic tropospheric chemistry”. Morgenstern et al calculated chemistry in the troposphere but the chemistry scheme excluded “higher” VOCs. Newman et al. simply imposed climatological distributions of species below 400 hPa. Answer: done

P17004L19: “did not simulate the evolution” Answer: done

P17007: No mention of photolysis here. Is the photolysis interactive, or do you use lookup tables? Answer: We have added a short description of the photolysis rate calculation procedure.

P17009L16: A contributing factor here may be that Morgenstern et al. made the increased halogens non-interactive with radiation, i.e., considered only the impact of ozone depletion on radiation, not that of the increased CFCs. This would result in a cooling of the stratosphere (as with CO<sub>2</sub>). Answer: agreed, the proper sentence is added to the text

P17009L23: “less than obtained” (spelling) Answer: done

P17010L16: “The average global ozone loss” Answer: done

P17010L19: Insert “,” after “As expected,”. Answer: done

P17011L10: “, as illustrated in Fig. 6” Answer: done

P17011: As noted above, I agree that indeed considerable climate change might have occurred in the absence of the MP. I just don’t believe most of the geographical details in the plots unless obtained by a more comprehensive (atmosphere-ocean-chemistry) model and ideally backed up by more results from other models. In the absence of this, at the very least a qualifying statement would be in order, to say that these results require further investigation because of the model limitations etc. Answer: we have added a statement to the conclusions

P17012L7: “smaller than 75\_” Answer: Done

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P17014L1: I don't doubt that there are substantial differences between the CCMVal CCMs. However, these are poorly documented as neither cloud liquid water, nor ice, nor precipitation were archived. I suggest dropping the statement. In assessing UV effects of ozone recovery in CCMVal models, please discuss Bais et al., ACP, 11, 7533-7545, 2011. Answer: done

P17014L20: "In the absence of the MPA" Answer: done

P17014L23: "the MPA" Answer: done

P17014L25: "In the absence of the MPA, we model substantial" Answer: done

P17014L28: "When the MP limitations are not. . ." Answer: done

P17015L11: ". . .in protecting the ozone layer and the Earth' climate." Answer: done

Figures: The contour plots would benefit from colour bars to make them more easily understood. Answer: We prefer to use marked lines instead of the just color patterns, we think the numbers on the contour plots work better.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17001, 2012.

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