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

**Anonymous Referee #1**

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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 impact these earlier

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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. 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 suggest 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.

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In summary, the paper is worth publishing subject to a revision of the section on surface climate change, possibly along the lines indicated above, and subject to a clarification about the specifications of the SOCOL model that make it superior to those used by Newman et al and Morgenstern et al.

Details:

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

P17004L10: “a threefold increase”

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.

P17004L19: “did not simulate the evolution”

P17007: No mention of photolysis here. Is the photolysis interactive, or do you use lookup tables?

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>).

P17009L23: “less than obtained” (spelling)

P17010L16: “The average global ozone loss”

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

P17011L10: “, as illustrated in Fig. 6”

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

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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.

P17012L7: “smaller than 75°”

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.

P17014L20: “In the absence of the MPA”

P17014L23: ”the MPA”

P17014L25: “In the absence of the MPA, we model substantial”

P17014L28: “When the MP limitations are not. . .”

P17016L11: “. . .in protecting the ozone layer and the Earth' climate.”

Figures: The contour plots would benefit from colour bars to make them more easily understood.

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

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