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Atmospheric Chemistry and Physics Discussions

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

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This manuscript describes climate response to MPA as simulated from a state-of-the art chemistry climate model. The topic is of interest and results are clear, however my feeling is that this work suffers from a lack of originality. Answer: We do not completely agree with this statement. Our paper presents original results obtained with the CCM SOOCL. Probably the object of the study is not new. The idea about the dangerous consequences of the halogen containing substances was proposed by Rowland and Molina many years ago and lead to the limitation on the ODS production. The benefits of this measure were estimated many times using simple and then more and more complicated models (Slaper et al., 1996; Prather et al., 1996; Egorova et al., 2001; Morgenstern et al., 2008; Newman et al., 2009). Our paper is a natural continuation of

the process.

1.The motivation for this analysis is not clearly discussed : the authors clarify that one important difference w.r.t. Newman et al 2009 or Morgestern et al. 2008 is that tropospheric chemistry is included in the SOCOL simulations whereas it was not in previous works; however this important difference or the possible effects on results are not discussed throughout the manuscript. Answer: We have added the discussion on the role of the tropospheric chemistry.

Lines 27-30 page 04: "some of the above mentioned feedbacks have been included": which ones? Could you be more precise? "therefore results could differ from Morgenstern and Newman." Not clear: they differ or they do not? If the model is different, I would expect results are different in any case, but I don't think this is the point of the authors.

Answer: We have rephrased this sentence.

Lines 1-2 page 05; "also our simulations cover. . . Newman et al terminated in 2065." This is not a scientific motivation. (Pay attention, Newman et al is 2009 and not 2010, unless you are referring at 2 different works)?

Answer: We think that the period of model integration is important. For example it allows understanding whether the effect can reach saturation or not. The reference has been corrected.

2. No changes in the dynamics are analysed in this work, I think that a general discussion on changes in stratospheric and/or tropospheric dynamics would render the analysis more comprehensive and interesting: For example, is the wind structure of the stratosphere-troposphere system affected in a no-MPA world? Are planetary waves affected by possible changes in winds? Is the BD circulation changing in a different way w.r.t. MPA simulations? What's happening to the polar vortex? Are SSWs occurring more/less frequently or changing statistics? No study on changes in variability is dis-

cussed: for example are there changes in the inter-annual variability ? In case, have these changes any impact on ozone?

Answer: The main subject of the manuscript is to analyze the benefits of the MPA. The analysis of the circulation response is interesting, but it is out of the scope of this particular paper and probably worth a separate paper. Anyway, we have added the analysis of several quantities such as zonal wind, BDC, NAM/SAM mentioned by the reviewer.

3. Impact at surface (Section 3.3):

- The simulations are carried out without an interactive ocean, I think this should be emphasized when looking at patterns of precipitation and tsurf.

Answer: We have emphasized this limitation in the conclusions.

- no mechanism is cited or hypothesed or reviewed to identify reasons of the impact of ozone changes at surface (are tsurf and precipitation changes consistent with changes in modes of variability such as SAM and NAM?).

Answer: The mechanism of the downward propagation of the stratospheric perturbation has not been clearly identified yet. We have added some results concerning NAM and SAM responses.

- what is the impact of the projected ozone changes on the eddy-driven jet in the SH? could results be in agreement with recent studies on the impact of ozone recovery and ozone depletion on tropospheric circulations and SAM (see Son et al., science, 2010 and Thompson et al. , Nat. Geos., 2011 for a review)? I think answering these questions could be very interesting.

Answer: The main subject of the manuscript is the response of the stratospheric ozone and surface UV radiation. The response of the tropospheric circulation is interesting, but it is out of the scope of this particular paper and requires deeper analysis and probably additional runs using a model with interactive ocean.

4. Section on the Erythemal UV irradiance estimations is interesting. Could the authors also refer to M.I. Hegglin and T.G. Shepherd. 2009, Nature Geoscience

Answer: We have added this reference. . -Why not adding a surface UV plot from SOCOL directly (which are the shortwave bands of its radiation code)? Would there be many differences in the UV at surface?

Answer: The radiation code of MA-ECHAM4 used in the model for the calculations of the heating rates and radiation fluxes has very rough spectral resolution and does not allow calculating Erytemal radiation? It is true for many CCMs, therefore the procedure of UV calculations using the output fields is considered as the best way to proceed (see Tourpali et al., 2009; Bais et al., 2011)

The authors specify that transient simulations with ref and world avoided scenarios of ODS were performed with several CCMs but those simulations have not been analyzed yet. It could be interesting extending this analysis to other models (a multi-model analysis) in order also to quantify which are the ranges of simulated no-MPA responses among models and if it could be possible to interpret differences between no-MPA and MPA fields w.r.t. internal variability.

Answer: The number of CCMs participating in this experiment is too low (3) to perform multi-model analysis.

A curiosity: in the title it is specified “benefits”, if the benefit is clear for the stratospheric ozone and UV at surface, it is not for changes in precipitation or tsurf.

Answer: The lower mixing ratio of ODS due to Montreal Protocol limitations can also be considered as beneficial because it should decrease the magnitude of greenhouse warming. Unfortunately, we cannot estimate the significance of this effect due to absence of interactive ocean. We have added this to the conclusions.

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