

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

### **Anonymous Referee #2**

Received and published: 10 September 2012

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.

Some major points are listed below:

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.

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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. 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)?

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

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.

- 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?).

- 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,

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2010 and Thompson et al. , Nat. Geos., 2011 for a review)? I think answering these questions could be very interesting.

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 ?.

-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?

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

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.

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

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