

Interactive comment on “Multi-model assessment of stratospheric ozone return dates and ozone recovery in CCMVal-2 models” by V. Eyring et al.

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Reply to Referee 2

We thank the reviewer for the constructive comments. The reviewer has pointed out several suggestions for improving the paper. We have considered carefully each of the comments and have modified the text accordingly. Detailed answers to the reviewer's comments are given below.

General Comments:

The paper reports on a series of coupled chemistry-climate model simulations designed to study ozone recovery under declines in ozone depleting substances and

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increases in greenhouse gas forcing. The authors have accumulated a good series of experiments and model fields to study ozone recovery in detail and this work is an important contribution to the science of ozone depletion. While I recommend that this work be published, there are a couple of areas that could be improved that I suggest below. In addition, there are recommendations for how the writing and clarity could be improved.

Major Comments:

The paper describes 'predictions' of ozone recovery. I would encourage the authors to reconsider using predictions in favor of projections. While this may not seem that important, the difference between these words is significant and I believe the paper reports on projections of ozone recovery.

We fully agree. We are not predicting the future, but are providing projections of the ozone layer under a certain future GHG scenario. The word 'predict' was mistakenly used one time in the submitted version (p. 11663; L. 11). This has now been replaced with 'project'.

On a related note, the simulations only report on a single GHG emission scenario. While acknowledged that different GHG emission scenarios would influence ozone, there was no effort to respond to how different scenarios would affect these results. This seems particularly important given that the A1B scenario considered is probably too low compared to our present emission trajectory. Because some areas of the stratosphere will recover in the next few decades, considering how additional GHG emissions would affect recovery even at a qualitative level would seem a valuable addition to this paper.

We agree that it is important to study the coupled chemistry-climate system un-

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der GHG scenarios other than SRES A1B. However, the CCMVal-2 reference simulations were made only for a single GHG scenario. A small set of four models recently performed simulations under GHG scenarios different from SRES A1B. They have been analyzed in an accompanying paper which we refer to (Eyring et al., GRL, 2010).

The other point I would like to make concerns describing model uncertainties. I think the authors should make further efforts to explain some of the model uncertainties present. For example, prescribed ocean/ice driving an atmosphere is one area. Also, were there multiple ensembles run for any of these simulations? It would certainly be worth a short discussion of how these different ensemble members agreed or disagreed with other members. I think this discussion would be useful to help the reader understand the value and uncertainties related to models of this type.

We have added a paragraph at the end of the paper that discusses model uncertainties.

Minor comments:

Table 1: Could one consistent unit be used? For example, T31, 200km, 1.9 degrees x 2.5 degrees. Also, 50 hPa, and 70km. Perhaps in parenthesis could be an approximate equivalent.

The difference in units here results from differences in model characteristics. We have added a sentence for clarification (T42 approximately corresponds to 2.8° x 2.8°, T30 to 3.75° x 3.75°.)'.

Figure 4/5. Should the order be switched for consistency?

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We have combined the REF-B2, fODS and fGHG simulations into one figure in the revised version, see below.

Some of the figures are really small. I hope this can be improved.

The quality of the figures has been improved for the final version. Since the 8-panel figures in the submitted version were very small, we have split them into extrapolar and polar and have also added the upper stratosphere in the polar regions. To further improve the quality of the figures, in the main paper we now only show the multi-model means while the figures that show the individual models have been moved to the supplementary material. This change significantly improves the figures.

P11662L15: We only expect this if parties continue to adhere to the agreement of the treaty.

Agreed and 'in future projections that are consistent with current restrictions imposed by the Montreal Protocol' added.

P11662L20: Perhaps a more recent reference about our expectations for future emissions could be used. Even IPCC 2007 has language describing our expectations.

We have added the IPCC (2007) and Moss et al. (2008) reference.

P11663L5: In CCMs, does dynamics also affect trace gas concentrations. If so, perhaps this should be mentioned.

Done.

P11663L11: Predict or project?

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Project is now used throughout the paper.

P11663L21: This passage is a bit confusing. Can you define WMO 2007? Also, can you describe the distinction between ozone recovery and ozone return more thoroughly here, as the description in the abstract should stand alone.

WMO (2007) defined and more information added on the difference between ozone return dates and full ozone recovery.

P11665L10: I'm not sure why this section is indented. Perhaps a leading sentence would help.

'Their main characteristics are summarized below:' has been added.

P11665L20-25: These sentences are a little confusing, perhaps some rewording would help.

Reworded.

P11667L3: The TSAM is not that clear and although further details exist in other references, it would help the reader for further explanation. For example, why use this technique instead of just an anomaly? What are the benefits?

Additional details on the TSAM method and its advantages have been provided.

P11669L10: Should this be CO₂ or GHGs. This appears other places as well, so if it is CO₂, perhaps this can be explained.

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The cooling is mainly caused by CO2. This has been clarified and a reference added.

P11669L24: This and the following are very long paragraphs and make the reading challenging. I suggest breaking these up into multiple paragraphs and perhaps even a separate subsection.

Section '4.1 Tropical ozone' has been further divided into three subsections 'Tropical upper stratospheric ozone', 'Tropical lower stratospheric ozone' and 'Tropical total column ozone'.

P11671L6: This seems an area where a natural section break could occur.

Done.

P11671L9: Reword sentence to describe how the figure shows tropical upwelling is a consistent solution of the models and how this would explain declines in ozone. The way it's stated here that isn't clear.

Paragraph has been reworded.

P11671L16: How do we know the primary contributor is SSTs?

This has been studied in the work that is cited and is supported by the fGHG simulation from the SOCOL model.

P11679L17: Should this be ? have similar means instead of from the same population?

No, this needs to say same population as done in the manuscript.

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P11684L4: Should this say, 'in agreement' instead of 'confirming'? Just because the two studies agree, doesn't mean it's confirmed.

Agreed and changed.

P11684L10: Seems the tense in 'occurred' needs to be changed.

Changed to 'full recovery of ozone from ODSs in this region of the atmosphere is projected to not likely have occurred by 2100'.

P11685L29: 'is the only one' This sentence doesn't make sense to me. Why is this the only plausible GHG scenario? Especially when at present we are currently above the A1B emission and concentration level.

Changed to 'not the only plausible GHG scenario for the future'.

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