

Interactive
Comment

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

Anonymous Referee #2

Received and published: 29 June 2010

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 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:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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. 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.

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.

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.

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

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

P11662L15: We only expect this if parties continue to adhere to the agreement of the treaty. P11662L20: Perhaps a more recent reference about our expectations for future

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

emissions could be used. Even IPCC 2007 has language describing our expectations. P11663L5: In CCMs, does dynamics also affect trace gas concentrations. If so, perhaps this should be mentioned. P11663L11: Predict or project? 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. P11665L10: I'm not sure why this section is indented. Perhaps a leading sentence would help. P11665L20-25: These sentences are a little confusing, perhaps some rewording would help. 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? P11669L10: Should this be CO₂ or GHGs. This appears other places as well, so if it is CO₂, perhaps this can be explained. 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. P11671L6: This seems an area where a natural section break could occur. 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. P11671L16: How do we know the primary contributor is SSTs? P11679L17: Should this be – have similar means instead of from the same population? P11684L4: Should this say, 'in agreement' instead of 'confirming'? Just because the two studies agree, doesn't mean it's confirmed. P11684L10: Seems the tense in 'occurred' needs to be changed. P11684L19: Ditto. P11685L29: 'is the only one' P11685L29: 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11659, 2010.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)