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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comment on "Tropospheric temperature response to stratospheric ozone recovery in the 21st century" by Hu et al. (2011)" *by* M. Previdi and L. M. Polvani

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We thank the referee for reviewing our manuscript. Below are responses to each point that was raised.

p2855, line 24 The Thompson and Solomon (2002) paper referred to is almost ten years old now and an update by Thompson et al, Nature Geosciences, 2011 shows quite a different signature of the SAM over parts of the Antarctic peninsula. I would suggest revising the paragraph to focus on the wider surface temperature features induced by ozone.

Response: This is a good suggestion which we will follow.

p2857, lines 5-8 This sentence could be construed as though stratospheric ozone forcing is the only difference between GROUP1 and GROUP2 in the A1B experiment, whereas there are likely many other differences in forcings between the two groups (eg. black carbon, land use, tropospheric ozone). It would be useful to highlight the global average and Arctic warming numbers in a table or as labels on the figure, as they get a little lost within the text. This is particularly true for the listing of DJF and JJA trends as 'per 50 years' for the A1B experiment and 'per decade' for the 1%/year CO2 experiment. That being said, a caveat to the study here is that the CO2-equivalent forcing between the 1%/year CO2 and A1B experiments are not identical and result in different rates of warming per decade from the forcing alone. While differences between the groups of models would minimise this issue, I'm not convinced that the similar global mean warming anomaly rates between the two experiments means that much. The similarity between the spatial patterns provides a convincing argument that CO2 is the culprit but the quantification of these differences is more difficult to attribute.

Response: We agree that there are other differences in the forcings between the GROUP1 and GROUP2 models besides stratospheric ozone, and we will make this point clear in the revised manuscript. We will also add a table indicating the global average and Arctic warming numbers as suggested. We thank the reviewer for the comment about interpreting the global mean warming anomaly rates in the two experiments. We agree that the more convincing argument for a different response of GROUP1 and GROUP2 to CO2 forcing is the similarity of the spatial patterns of warming between the A1B and 1%/year CO2 experiments.

p2859, line 1-3 While I understand the authors want to make a clear statement against the assertion of the Hu et al study, the line 'rather than to any effect of ozone recovery' seems overly strong, as, how can we be sure? The authors may wish to comment that until we have ozone-only forcing experiments (similar to McLandress et al 2011a) from multiple models, it will be difficult to completely attribute the impact of stratospheric ozone on climate.

ACPD

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Interactive Comment

Response: This is an excellent suggestion which we will follow.

p2859, line 13 Add ‘surface’ to the last sentence ie. ‘global surface warming’ as that is the only aspect of the Hu et al study that the comment has addressed.

Response: This will be done.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 2853, 2012.