

## ***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***

**Anonymous Referee #4**

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Review of Previdi and Polvani: “Comment on Tropospheric temperature response to stratospheric ozone recovery in the 21st century” by Hu et al. (2011)”

The authors present an analysis of 1%/year increasing CO<sub>2</sub> simulations to suggest, very convincingly, that differences in warming between two groups of models that were attributed by Hu et al (2011) to stratospheric ozone, are most likely due to differences in the response to CO<sub>2</sub>. The comment is very well-written and conceived and I find it acceptable for publication with only very minor comments.

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

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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.

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 CO<sub>2</sub> experiment.

That being said, a caveat to the study here is that the CO<sub>2</sub>-equivalent forcing between the 1%/year CO<sub>2</sub> 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 CO<sub>2</sub> is the culprit but the quantification of these differences is more difficult to attribute.

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.

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.

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