Atmos. Chem. Phys. Discuss., 11, C15388–C15393, 2012
www.atmos-chem-phys-discuss.net/11/C15388/2012/

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* C. McLandress et al.

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Received and published: 16 February 2012

Reply to Reviewer 3

We thank the reviewer for his or her comments, which are repeated below in italics.

This study presents a likely alternative to the conclusions of Hu et al 2011 on the impact of stratospheric ozone changes on tropospheric temperatures. They use experiments with a single model and find no impact of stratospheric ozone on tropospheric temper-

atures, unlike Hu et al who found a significant warming. They conclude that differences in climate sensitivity between two groups leads to the significant temperature impacts found by Hu et al 2011. I find the paper acceptable for publication subject to the following comments and minor revisions.

The authors argue that the difference in tropospheric warming between the two sets of CMIP3 models in Hu et al 2011 is due to different climate sensitivities of the models. The overall tone of these statements is quite strong given the study does not directly analyse the CMIP3 models. An obvious way of checking this would be to determine the transient climate response from the 1CO2 runs of the CMIP3 models used by Hu et al 2011, rather than relaying information from a Table in the IPCC 4th Assessment Report that does not include all models. A recent paper submitted to ACPD (Previdi and Polvani, Atmos. Chem. Phys. Discuss., 12, 2853-2861, 2011) does such an analysis and the discussion of climate sensitivity should refer to that study.

We agree and the study by Previdi and Polvani is now referenced in the conclusion section.

Commenting on one study which subselected models with analysis of a single model is rather limited as both are open to criticism. Could not results from other simulations (eg. CAM3 ozone and GHG experiments of Polvani, Previdi and Deser GRL, 2011) be included in this study? Similar to the collaboration of Kang et al, Science, 2011 who used both CAM3 and CMAM experiments.

A proper assessment of the impact of ozone depletion/recovery on NH temperature trends requires coupled models. Including results from a model like CAM3, which uses prescribed SSTs, therefore would not be appropriate. We agree that the use of a single model for a study like this has its drawbacks (as we stated in our conclusions), but we

had no choice since CMAM is presently the only CCM with a coupled ocean.

Another possibility for differences between the two sets of CMIP3 models may be the inclusion of tropospheric ozone forcing in the models that include ozone changes. Presumably the CMIP3 models without ozone included neither stratospheric or tropospheric ozone changes. The additional of tropospheric ozone, a greenhouse gas, would likely lead to warming in the troposphere. Can the authors comment on this?

Tropospheric ozone is expected to increase from 2000 to 2050 if prescribed according to the SRES AB1 scenario and thus provides a positive radiative forcing. According to Table 10.1 in the IPCC AR4 report, most but not all models that include stratospheric ozone changes also include tropospheric ozone changes, and vice versa. So this may indeed be a contributing factor in explaining the result of H11, and we have added text to that effect. However, the analysis of Previdi and Polvani shows that incomplete removal of differences in the response to CO2 forcing is probably the dominant factor behind the H11 result.

The number of figures seems a little excessive for a comment. One suggestion would be to summarise the timeseries figures in a table.

We have removed the two figures showing time series, which admittedly do not add much to the other figures. A table would take up a lot of space too and since we are not really interested in the specific numbers (most of which are not statistically

significant), we actually don't see the value in a table.

Minor comments:

- the CMIP3 model simulations are variously referred to as IPCC models or AR4-models. Suggest using CMIP3 models throughout.

We had been following Hu et al. in referring to the models as IPCC-AR4 models, but we agree that the correct designation is really CMIP3.

- REF-B2-GHG could be interpreted as REF-B2 with GHG only. Suggest changing to REF-B2-minus-GHG

There was only one occurrence of REF-B2-GHG. The sentence has been reworded to avoid confusion.

p32994, line 20: change 'stratospheric cooling' to 'stratospheric depletion'

The reviewer's suggested wording is not right, but we think we understand the problem with our wording (i.e., cooling causing cooling). The revised phrase is "long-wave cooling due to reduced downwelling infrared radiation from the colder stratosphere."

p32994, line 23-p32995, line 1: clarify that these studies are looking at the impact of

ozone on the tropospheric circulation

Done.

p32996, line 2-3: this statement needs backing up with a reference as to my knowledge it has not been shown that the impact of stratospheric ozone changes on tropospheric temperatures is limited to the Antarctic region.

This is simply a statement of common sense, and we have no supporting reference for it. As the reviewer well knows ozone depletion has had a strong impact on summertime SH tropospheric climate, as has been documented in numerous studies. The fact that there are no such studies for the NH troposphere indicates that there is no noticeable impact of stratospheric ozone depletion there, as was concluded in Chapter 4 of WMO (2011).

Perhaps it was our wording "everywhere outside the Antarctic" that is problematic, since it might be construed as meaning a narrow boundary between large and small impact. We have reworded that sentence to better convey our meaning.

p32997: delete 'which is in fact opposite in sign to theirs' given the change is likely insignificant

Although the NH tropospheric temperature trend due to ozone recovery is small, it is nevertheless statistically significant, as can be seen in the bottom right panel of Figure 2. It is therefore justifiable to state that it is opposite in sign to theirs.

p32998: note here that other forcings are included (eg. aerosols) that may contribute

to the differences

Aside from the ODS forcings, all other forcings (including aerosols) used in the REF-B2 and GHG simulations are identical. Thus, differences between the two simulations can only arise from the ODS forcing (and, of course, climate "noise").

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32993, 2011.