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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* C. McLandress et al.

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Reply to Reviewer 1

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

General comments:

This manuscript comments on the paper of "Tropospheric temperature response to stratospheric ozone recovery in the 21st century" by Hu et al., which showed that

stratospheric ozone recovery may enhance greenhouse warming in the troposphere and on the surface, based on IPCC-AR4 simulations for the 21st century. To compare with Hu et al.'s results, the authors performed two sets of simulations with a coupled atmosphere-ocean GCM (also with interactive ozone chemistry): one has both increasing GHGs and time varying ODSs, and the other one has increasing GHGs. They found that the difference of tropospheric temperature trends between the two sets of simulations is weaker than that in Hu et al. and has opposite sign. They thus suggest that the results in Hu et al. are likely due to different climate sensitivities of AR4 models, rather than real signals of stratospheric ozone forcing.

The magnitudes of temperature trends differences between their two groups of models are certainly debatable (it could be due to different model sensitivities to both GHGs and ozone, different forcings in AR4 models such as black carbon, and so on), as pointed out by Hu et al. themselves. Therefore, I think that the paper is acceptable for publication. On the other hand, it will be more convincing for the present manuscript to address the following questions.

If Hu et al had felt strongly that the magnitude of the temperature trends between their two groups of models was debatable, then we wonder why they submitted the paper in the first place, let alone attribute specific trend values to ozone forcing in their Abstract and Conclusions.

Specific comments:

1. The sign of stratospheric ozone forcing: According to previous results in radiative convective models (e.g., Ramanathan and Dickinson, 1979, Forster and Shine, 1997, Hu et al., 2011), the radiative forcing of stratospheric ozone on the troposphere is positive. That is, ozone depletion causes cooling in the troposphere, and increasing ozone leads to positive forcing. The sign of tropospheric temperature trend differences *in Hu et al. is consistent with that in radiative-convective model for both periods of 1965-1999 and 2001-2050 (see Figure 2 in Hu et al.), although the magnitudes are debatable. Then, the question is why the coupled GCM simulations here generate opposite signs against that in these radiative-convective models. Are there any negative feedbacks that reverse the sign of ozone forcing in the GCM?*

As we stated in our text and as is demonstrated in Figure 10.21 of SPARC CCMVal (2010), there is a significant compensation in the ozone radiative forcing between short-wave warming and long-wave cooling, and the sign of the net forcing is uncertain. Several CCMs in that figure show a positive net radiative forcing, and IPCC (2007) quoted an ozone radiative forcing of -0.05 +/- 0.1 W/m2, explicitly acknowledging the possibility of a positive net radiative forcing. So the reviewer is incorrect to imply that the ozone radiative forcing must be negative. We would furthermore note that we only find statistically significant temperature changes for the NH, not for the global mean. As the reviewer correctly notes, it is also possible that circulation feedbacks could lead to a difference in the surface temperature changes from that predicted by the radiative-convective models, and we have added a sentence to that effect. However, to get into these issues in detail is beyond the scope of our paper: we do not wish to emphasize the sign of our surface changes, rather their magnitude and the nature of their hemispheric asymmetry.

2. I feel that the results in Hu et al. cannot be simply attributed to climate sensitivity of models to GHG forcing. From Table 1 in Hu et al., one can find that the two groups of models are almost the same for both 21st and 20th century simulations (except for two GISS models). If the group of models with ozone recovery has greater model sensitivity to GHGs, it would also cause greater warming trends for 20th century simulations. However, Figure 2b in Hu et al. shows weaker warming trends over the period of 1965-1999. How to explain this? In addition to model sensitivity to GHGs, model sensitivity

to stratospheric ozone may also be important, I think.

By "climate sensitivity" we mean the overall sensitivity of a model to increasing GHG concentrations, which includes the high-latitude sensitivity (see page 33002 of the submitted manuscript). The reviewer is referring here to the "transient climate response", which is a global metric and does not capture the high-latitude sensitivity. Since there is no 1:1 relationship between a model's TCR and its "Arctic amplification factor", comparing the TCRs, as the reviewer is doing here, is not meaningful.

In any case we were unable to reproduce the numbers produced in Table 1 of Hu et al. from the information provided in IPCC (2007), and the sets of models used for the past and the future runs were not identical.

A nice demonstration of our speculation that the Hu et al results are attributable to the climate sensitivity of the models to the GHG forcing has recently been provided by Previdi and Polvani (ACPD, 2012). They use the same set of models as Hu et al, but analyze an experiment in which CO2 increased by 1% per year, with stratospheric ozone forcing held fixed. They show that the surface temperature response, computed by differencing the same two sets of models used by Hu et al, is remarkably similar to the Hu et al u et results, indicating that the temperature enhancement found by Hu et al is a consequence of differences in the response to GHG forcing, and has nothing to do with ozone recovery. In our Conclusions we now refer to Previdi and Polvani (2012), and say how this is a clear demonstration of our speculation about the Hu et al results.

3. I agree with the authors of the manuscript that multi-model simulations are neces-

sary to clarify the issue here.

No comment.

4. Figures are not clear enough.

The reviewer has provided no reason why the figures are not clear enough nor made any suggestions as to how to improve them. Reviewer 2, however, did suggest ways to modify the figures so as to better emphasize the important features. We have therefore followed that reviewer's advice.

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