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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing since preindustrial times due to ozone change in the troposphere and the lower stratosphere" *by* M. Gauss et al.

M. Gauss et al.

Received and published: 6 December 2005

First of all, we would like to thank the two referees for their very helpful comments and suggestions. In the following we will address in detail all remarks given by the reviewers. The reviewer comments will be given in quotes "", our answers will be marked by –>

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Anonymous Referee #1 Received and published: 31 August 2005

–> We have included all studies mentioned by reviewer #1 in the list of references:

Isaksen I.S.A., C.S. Zerefos, K. Kourtidis, C. Meleti, S.B. Dalsoren, J.K. Sundet, A. Grini, P. Zanis, D. Balis, Tropospheric ozone changes at unpolluted and semipolluted regions induced by stratospheric ozone changes, J. Geophys. Res., 110(2), doi: 10.1029/2004JD004618, 2005.

Stolarski, R., R. Bojkov, L. Bishop, C.S. Zerefos, J. Staehelin, J. Zawodny, Measured trends in stratospheric ozone, Science, 256, 342-349, 1992.

Zerefos, C.S., Long-term ozone and UV variations at Thessaloniki, Greece, Phys. and Chem. of the Earth, 27, 455-460, 2002.

The three papers are now referred to and shortly discussed in the first paragraph of the introduction. The main points mentioned are the ozone decrease in the stratosphere found from long-term measurements of column ozone and its implications for UV change and tropospheric chemistry change in the lower atmosphere.

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Anonymous Referee #2 Received and published: 29 September 2005

"The model results shown are valuable and clearly presented but the analysis needs to focus more precisely on the ozone signal and associated radiative forcing. As it is, section 2 includes many statements about dynamical/transport processes which are not supported by the global mean diagnostics presented in this study."

–> Following the reviewer's suggestions below, explanations on the ozone decrease in the tropical lower stratosphere and the decrease of the tropospheric ozone column have been added. Each time an explanation is only tentative rather than deduced from the analysis, this is now made clear by writing 'probably', 'it is assumed', etc. An additional study has been made with DLR to analyze changes in stratospheric circulation and downward cross tropopause flux of ozone (see 3rd paragraph of section 2.3.2).

"Would suggest to summarize the main results at one single place. The information in sections 2.4 and 3.4 could be moved in section 4 and elsewhere. The information in

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section 4 should focus on the details of the model results."

–> Agreed. The major part of the old sections 2.4 and 3.4 is now incorporated in section 4. The small remainder is added partly to section 3.2 (last paragraph) and partly to section 3.3 (last paragraph).

"page 11 - first paragraph: It is not convincing that ozone biases for UIO_CTM2 are restricted to low latitudes. Figure 2 indicates large ozone losses at higher latitude throughout the year in both hemispheres."

–> Our formulation was misleading. We did not mean to say that ozone depletion in UiO_CTM2 is _restricted_ to low latitudes. Rather, it is in low latitudes that this depletion is larger than what has been observed and larger compared to most of the other models. However, we found a mistake in the model code and the UiO_CTM2 results are updated in the revised manuscript. The too strong ozone depletion in low latitudes is not seen anymore and the statement in question has been removed. The fact that ozone depletion is underestimated by the new UIO_CTM2 calculation in high latitudes is now mentioned explicitly (last sentence of 1st paragraph in section 2.3.1).

"page 12 - Last sentence: This is a surprising comment since the majority of models show an ozone increase below 10km in both hemispheres. The authors might consider showing horizontal maps of column ozone to support it."

–> The tropospheric ozone column decreases are regionally confined and therefore not necessarily seen on zonal-mean plots. Only in the case of the ULAQ and NCAR_MACCM models they extend along the entire latitude circle. In the revised manuscript this statement is made clearer (see discussion of Figure 3 in section 2.3.1).

"page 13 - 2nd paragraph: About UIO_CTM2 -> see previous comment."

–> The UIO_CTM2 results have been updated, and the statement about low latitude ozone depletion is removed. As mentioned above, the underestimation of high latitude ozone depletion by UIO_CTM2 is now mentioned in the discussion of Figure 2.

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“Page 13 - 3rd paragraph: This paragraph is far too long and repetitive and the discussion refers to table 5 without mentioning it. Also, ULAQ appears to be the only model showing a significant impact of the halogen chemistry on the tropospheric signal (-2.6 vs 7.9) whereas the last sentence suggests a more robust signal from the different models.”

–> In the revised manuscript this is the last paragraph of section 2.3.1: The reference to Table 4 (=Table 5 in old manuscript) has been included. In the old manuscript, the paragraph had 17 lines. Following the reviewer’s suggestion it was reduced. However, after the resubmission of DLR_E39C’s new results the consistency between the models’ results was reduced, and two sentences had to be added, trying to explain this. The disagreement among the models regarding the magnitude of the signal from the tropospheric change is now better emphasized by the addition ‘Ĕalthough the magnitude of the effect varies’.

“page 15 - First paragraph: It may not be necessary to refer systematically to ozone changes as a perturbation on the top of another perturbation. For example, the discussion about Figure 5 could be referring to ozone decrease/increase instead of "tropospheric increase becomes larger". Such substitution there and elsewhere in the text when appropriate would facilitate the reading.”

–> The discussion of Figure 5 (3rd paragraph of section 2.3.2) has been edited according to the reviewer’s suggestion (one exception is when we mention a ‘less severe ozone reduction’ towards the end of the paragraph, since the formulation ‘ozone increase’ would be misleading in this case) . Also elsewhere the suggested substitutions have been made where appropriate.

“page 15 - line 3: Would be more specific about the impact of increase meridional circulation on ozone changes in different regions. For example, DLR_E39C shows an ozone decrease throughout the lower stratosphere which will not necessarily lead to ozone fluxes increase in the extra tropical region.”

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–> An additional analysis has been made to quantify this at the example of the DLR_E39C model. This is now included in the manuscript (see 3rd paragraph of section 2.3.2).

“page 15 - line 6: For DLR_E39C, I would replace : "more depletion in the lower tropical stratosphere" by "ozone decrease in the lower stratosphere" since the signal is not restricted to the tropics.”

–> Done.

“page 15 - last sentence : The author relates the ozone signal on Figure 5 caused by the SST forcing (1c-1) with the temperature changes on Figure 4 (2-1) which is caused by chemistry+SST. So, I would not directly associate ozone increase on figure 5 to stratospheric cooling. Unless the authors refer to Figure 2? please clarify.”

–> We agree that relating Figure 5 to Figure 4 requires the figures to be consistent. Therefore, Figure 4 has been changed to show ‘1c-1’ in line with Figure 5. The temperature change due to SST+WMGHG (‘1c-1’) is not very different from the temperature change due to SST+WMGHG+chemistry (‘2-1’). The temperature decrease in the stratosphere is about 1 K less when ozone reductions are not taken into account (‘1c-1’), but there is still a significant cooling. It is therefore assumed that temperature change contributes to ozone increase in the middle stratosphere. The second paragraph of section 2.3.2 has been edited according to the changed figure. In connection with this, a typo was found in the caption of Figure 5: For UM_CAM ‘1b-1d’ is shown, not ‘1d-1’. Typo removed.

“page 16. - section 2.4 - ’An important finding...’: Only need to mention the significance of the climate forcing on the results.”

–> The contents of section 2.4 have been moved to section 4. The expression “An important finding” has been removed. The significance of climate change for ozone change and radiative forcing is explicitly mentioned. See second and forth paragraphs

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of section 4.

“page 19. - section 3.1.2 - last sentence: It underlines the importance of surface forcing (not necessarily STE).”

–> Agreed. This sentence has been removed. The strength of the radiative forcing calculated in this study is merely a result of ozone changes. The ozone changes, in turn, are influenced by surface forcing, STE, lightning, etc., and this is discussed in section 2. There it is stated more clearly what might be due to changed emissions and changed climate.

“page 24. First sentence: Figure 2 shows mainly an ozone decrease (not increase?) throughout the lower stratosphere for all models - clarify. Also, the impact of "transport of enhanced tropospheric ozone and precursors" has not been clearly described in section 2 (almost appears like a new element in the study).”

–> Agreed. This sentence was not clear and parts of it must be mentioned in section 2. Most of the models with explicit stratospheric chemistry (for which stratospheric RF is shown in Figure 7) yield an increase of ozone in the lower _tropical_ stratosphere (as can be seen in Figure 2). The emphasis, of course, has to be on ‘tropical’, which unfortunately was omitted in the sentence. It is now added in the sentence referring to this change (last sentence of section 3.2.1). Globally averaged, the stratospheric RF is slightly positive in two models (DLR_E39C and FRSGC_UCI) due to the large area weight of the low latitudes and the relative importance of the UTLS ozone increase for the LW signal. Upward transport of ozone and ozone precursors is a possible cause of the ozone increase and is now already mentioned in section 2 (rather than section 4) with two sentences starting at line 6 of section 2.3.1.

“Table 4 is not really necessary.”

–> Agreed. Table 4 removed.

“Figure 4 could be removed or replaced by one panel showing annual mean profiles for

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the various models."

–> The figure has been replaced by annual-mean profiles for the various models. The discussion of Figure 4 (second paragraph of section 2.3.2) has been edited accordingly.

"Figure 7 : the choice for the color scale is not appropriate to differentiate positive/negative radiative forcings."

–> Figure 7, and also Figure 6, have been improved in this respect. Positive and negative forcings are now clearly separated.

"Information in table 1 could be condensed. Informations about transport schemes, GWD and convective schemes could be mentioned in the text when needed."

–> The information on GWD has been removed. The advective and convective schemes, however, are still included in the table because we consider them very fundamental features of both CTMs and GCMs, and important for judging the differences between the models.

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Some errors, not pointed out by the reviewers, were found in the online 'print version' but were corrected in the revised manuscript:

The DLR_E39C and UIO_CTM2 models updated their results, and the radiative forcing calculations were redone. In the manuscript, the numbers concerned have been updated, but this did not change the main conclusions. In addition, the following minor changes have been made:

In the affiliations, 'Climate Change Unit' appeared twice. Removed one occurrence.

The last sentence of section 3.1.1. was corrected from 'This value agrees well with the estimate of 0.35 Wm-2 suggested by IPCC-TAR (2003).' to 'This value agrees well with the estimate of 0.35 +- 0.15 Wm-2 suggested by IPCC-TAR (Ramaswamy et al., 2001).'

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Univ. Reading has calculated adjusted radiative forcing. Table 8 now shows adjusted forcing (rather than instantaneous forcing) for consistency with other RF calculations from UiO-RTM, ULAQ, and Harvard GISS.

Updated Dentener et al. 2005 reference, which is now submitted to Environ. Sci. Technol.

Updated Dameris et al. 2005 reference, which is now published in ACP.

Included Kinne et al. 1992 and Stenke and Grewe, 2005 in list of references.

A typo was removed in figure caption of Figure 5: For UM_CAM '1b minus 1d' is shown (not '1d minus 1').

One acknowledgement was added.

Sincerely,

Michael Gauss on behalf of all co-authors

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5751, 2005.