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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of high solar zenith angles on dynamical and chemical processes in a coupled chemistry-climate model" *by* D. Lamago et al.

D. Lamago et al.

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Impact of high solar zenith angles on dynamical and chemical processes in a coupled chemistry-climate model by Lamago et al.

We thank both referees for their constructive and helpful comments. Based on these comments we feel that the paper has been significantly improved. Most comments and remarks including the comment made by Adrian Tuck have been considered in the manuscript. In the following, we give our remarks and comments on each point.

Reply to the comments of referee #1:

Major points:

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1. The parameterisation of photolysis frequencies. The text (after Eq. (6) until the end of this section) has been re-written to make it clearer (hopefully). We have tried to explain the parameterisation somewhat more in detail and to avoid misunderstandings.

2. a). How realistic are the simulated ozone fields? A discussion of the results has been added at the beginning of sub-section 3.2.1; since the validation with observations (Bojkov et al., 1999) was already given in Hein et al. (2001), only a short summary has been given here.

b). ... chlorine deactivation starts in mid October - is this realistic? From Figures 11, 12, and 13 one cannot (!) conclude that chlorine deactivation starts in mid October. In these figures we show differences between the two simulations. In SZA87.5, the deactivation in the South Pole area (i.e. south of 80°S) at 50 hPa begins around October 20th, but at lower latitudes chlorine deactivation begins much earlier, for example in mid September at 65°S (see Schnadt, 2001 (PhD thesis); her Figure 6.12). This is in good agreement with Grooss et al. (1997; their Figure 2).

c). ... chlorine deactivation seems to happen via conversion of ClOx to ClONO2. In E39/C, the chlorine deactivation in the Antarctic stratosphere clearly starts via conversion of ClOx to HCl. It is in reasonable agreement with Douglass et al. (1995) and Grooss et al. (1997). This has been shown first in Steil et al. (1998). The re-formation of the chlorine reservoir, and in particular the inter-hemispheric differences between the Arctic and Antarctic stratosphere are realistically reproduced by E39/C (see also Hein et al., 2001, and Schnadt et al., 2002). A sentence has been added at the end of Section 3.4 to make this clear.

3. ... earlier appearance and longer lifetime of the ozone hole ... An extra figure (new Fig. 6) has been included which zoom out the result of the changed lifetime of the ozone hole when considering twilight photolysis. Some text discussing the results more precisely has been added (middle part of sub-section 3.2.1).

4. ... midlatitude NOx. Does this effect likewise appear in the CCM results presented

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in the manuscript? Could a different NOx impact midlatitude ozone columns? Yes, midlatitude NOx is enhanced in SZA93 and therefore has an impact on ozone there. A discussion on this issue has been added in Section 3.2.1 (middle part); it is put together with a more detailed discussion of the results presented in Figure 5 (comment 8. of referee #1).

5. ... box model calculations cannot fit observed ozone loss rates ... We agree with this comment. We have changed the text (last part of third paragraph in Section 3.2.1) and we have added the references of Hansen and Chipperfield (1999) and Becker et al. (1998), as proposed.

6. ... equivalent PV coordinates ... We have now explained this somewhat more exactly. The text should be clearer now.

7. As proposed by the referee, we have re-written the last part of Section 3.4.

8. The statistically significant changes in total ozone north of 50°S have now been discussed in Section 3.2.1 (middle part). A sentence about it has also been included into the conclusions.

9. The former ('old') Figure 6 has been dropped. A new Figure 6 has been included (see comment 3.) to be able to describe the temporal evolution of the ozone hole in more detail.

Minor points:

1. The title has been slightly modified. Generally, 'high SZA' has been changed to 'large SZA' in the whole manuscript.

2. Has been changed.

3. Description of boundary conditions of GHGs has been slightly modified.

4. 'chlorine products' replaced by 'chlorine compounds'.

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5. The two sentences were misleading. Therefore they have been re-written.

6. A small addition has been made in the figure description. SZA greater 93° was chosen as the cut-off.

7. These points mentioned here have been/will be changed by the Editorial Office to keep ACP standards. We have done our best do fulfil these standards.

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Reply to the comments of referee #2 (Piers Forster):

General comments:

1. Yes, the model results, in particular climatological means of ozone and temperature, seems to be partly worse in SZA93. We have added some sentences in sub-section 3.2.1 (end of first paragraph) and at the end of the conclusions discussing this and to point out the improvements and degradations. The general statement is that an obvious improvement of a 'parameterisation' does not necessarily result in a general improvement of results. Additionally, in Section 3 the differences between SZA93 and SZA87.5 have been discussed more precisely, particular with regards to observations.

2. High SZA (larger 90°) are not considered in E39/C for the calculation of the heating rates. We agree with Piers Forster's comment that it would be interesting to determine this effect separately. Certainly, extra-solar heating could partially compensate enhanced ozone destruction. An assessment of this effect using the fully coupled model would require significant changes in the model and non-negligible computer power. Therefore, we have decided not to carry out these additional calculation now, but will keep this issue in mind for future sensitivity studies. Hopefully Piers Forster will agree on this. We have added a comment on this in the conclusions (beginning of second paragraph) to clarify that large SZA are not considered for heating rate calculations in a consistent way, and have discussed possible consequences for the model results.

3. Cause and Effect problem: Certainly, additional so-called off-line simulations (i.e.,

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run the model in a CTM-like mode of the model with fixed ozone for the calculation of heating rates) would make it possible to estimate changes of the photolysis rates that are only due to differences in the radiation scheme (i.e., largest SZA allowed). The results presented in Figure 2 show J-value differences which are due to both ozone differences and differences in the radiation scheme (SZA87.5 and SZA93). We do not expect a qualitatively different result compared with Figure 2, if we would did such off-line calculations. From our point of view, the expenses which would be necessary to obtain this additional information are not justified: at minimum, we would need two simulations of at least 1 year each with E39/C. We believe that the results presented in Figure 2 are reasonable, displaying the total effect. Generally, it is too 'expensive' to employ complex model systems for sensitivity tests which can be done more easily with 'simple' models. We therefore propose to renounce on additional sensitivity tests with E39/C and keep the discussion of results as it is.

Specific and technical comments:

1. A sentence has been added at the end of the abstract.

2. "some CCMs" has been changed to "most CCMs". We know only one CCM which considered large SZA (UMETRAC) in recent simulations. Other modelling groups will change it soon or have changed it very recently (pers. communications).

3. Has been corrected. Table 1 should be clickable now.

4. Wording has been changed. Hein et al. (2001) has been mentioned, since there it was explained in more detail (last part of Section 2.3).

5. A new Figure 6 has been added (first paragraph of sub-section 3.2.1) to show changes more clearly. (See reply to comment 3 of referee #1.)

6. This paragraph has been modified to make things clearer. a). "however" has been dropped. b). Sentence has been dropped. c). Text has been changed. d). Correct! Has been changed.

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7. 'applied CCM' has been changed to 'E39/C'.

8. Was misleading, has been changed.

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Comment made by A. Tuck:

Some sentences with regards to the importance of NIR photolysis have been added in the conclusions (last part of second paragraph).

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3681, 2003.