

Interactive comment on “Objective assessment of ozone in chemistry-climate model simulations” by A. Yu. Karpechko et al.

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We thank Reviewer for his/her detailed comments on our manuscript. Below are our responses.

1. "Specific Comments: Title and abstract: There is only one diagnostic that is global, which is the monthly mean total ozone climatology for 1980-84 compared to observations. Overall, the focus is on the southern hemisphere polar regions which is also mentioned somewhere in the paper, but this should be reflected in the title and abstract. I also think that the word 'objective' should be deleted from the title and in the text. This is not a more objective evaluation than previous studies."

We agree with both suggestions and change the title to 'Quantitative assessment of

Southern Hemisphere ozone in chemistry-climate model simulations'. We also emphasize in the abstract that our focus is on the Southern Hemisphere by revising the first sentence: 'Stratospheric ozone recovery in the Southern Hemisphere is expected to drive pronounced trends in atmospheric temperature and circulation from the stratosphere to the troposphere in the 21st century; therefore ozone changes need to be accounted for in future climate simulations.'

2. "It would be good if the goal of the study is mentioned somewhere in the abstract."

In the revised abstract the goal is formulated as follows: 'Many climate models do not have interactive ozone chemistry and rely on prescribed ozone fields, which may be obtained from coupled chemistry-climate model (CCM) simulations. However CCMs vary widely in their predictions of ozone evolution, complicating the selection of ozone boundary conditions for future climate simulations. In order to assess which models might be expected to better simulate future ozone evolution, and thus provide more realistic ozone boundary conditions, we assess the ability of twelve CCMs to simulate observed ozone climatology and trends and rank the models according to their errors averaged across the individual diagnostics chosen.'

3. "Section 1 Introduction p. 19353, l. 5: impacts on tropospheric circulation: this could be a bit more specific; please add Son et al. GRL (2009) to the reference list."

Done. The paragraph is rewritten as follows: 'Stratospheric ozone is expected to recover during the 21st century as a result of declining halogen abundances (WMO, 2007) and ozone recovery is expected to influence the position and strength of SH tropospheric westerlies, likely reversing the strengthening of westerlies caused by ozone depletion (Thompson and Solomon, 2002). However due to continuing increase of the greenhouse gas concentration, which act to further strengthen westerlies (e.g. Fyfe et al. 1999), the net change to the tropospheric circulation is still a subject of research (Miller et al., 2006; Perlwitz et al., 2008; Son et al., 2008, 2009). This implies that details of the ozone recovery need to be predicted well in order to reliably simulate future

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SH climate.'

4. "l. 18: remove the word 'recovery' after 'ozone'"

Done

5. "p. 19354, first paragraph: it should be made clearer that this study really is complementary to WE08 and follows a different approach. In fact, WE08 argue that looking at ozone is not permissible, since this is the quantity of interest, and instead they focus on the processes that determine ozone; this should be better reflected in the text here, e.g. change l. 3: 'however they did not' with 'because of that they did not,'; please also remove the word 'objective' here both for your study and for WE08. Quantitative might be better."

Following the suggestions, we changed the word 'objective' to the word 'quantitative' where appropriate, replaced 'however they did not' with 'because of that they did not', and also added a sentence saying that our study may be considered a complementary to WE08.

6. "Section 2 Data In previous CCMVal-1 studies 13 CCMs participated; why are only 12 CCMs used here?"

The data of AMTRAC model were not used because model's PI asked us to exclude the model from the evaluation.

7. "Section 3 Method Change 'Data set' to 'observations' in line 26 on page 19355 and line 7 on page 19356"

Corrected

8. "p. 19357, l. 6: instead of 'later', please specify the section."

Corrected

9. "line 6/7: 1980-1984 should not be called a pre-ozone hole climatology since ozone

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loss has already started (see you Figure 1)" This period is the best we have to assess undisturbed ozone climatology. In the revised version we took away the original statement and instead added a sentence clarifying that we realise that some ozone loss was present already during this period: 'The period 1980-1984 is meant to represent pre-ozone hole climatology, although some ozone loss took place already then (see Figure 1).'

10. "l. 25: ': : reasonable to expect that the models simulate the NH trends over 20 years correctly'"

Corrected

11. "Table 2 could be improved if the region where the diagnostics are applied is added in the first column (e.g. global for 1st, SH for 2nd and polar SH for the last two diagnostics);"

Done. Thanks for this suggestion.

12. "Can the uncertainty in the observed variable (σ) be added as an additional column?"

Please note that we do not use a single uncertainty value per diagnostic. For each diagnostic, the uncertainty is function of time (month) and space (latitude or pressure).

13. "p. 19358, l.5: the authors say that they want to put more weight on the model skill to simulate Antarctic ozone. I think the authors need to make a decision (see also above comment on title) whether they want to focus on global ozone or on Antarctic ozone. If the latter, then it might be better to also apply the diagnostic for the total ozone climatology (which is currently the only diagnostic that is applied globally) only over the region 60-90_S."

As we state in the text, the choice of diagnostics is subjective. Global ozone distribution is driven by processes that will likely contribute to future Antarctic ozone evolution (e.g. ozone production in the Tropics). Since we aim to infer from this assessment

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the ability of models to predict future ozone, we think it is important to include also a global diagnostic. A similar approach was chosen e.g. by Connoley and Bracegirdle (2007) who concentrated on the Antarctic climate but also assessed models skills in simulating the global climate. Also, we weight Antarctic ozone profile errors by ozone values; therefore the Antarctic (60-90S) total ozone climatology and trend diagnostics suggested by the reviewers would largely duplicate the corresponding profile diagnostic. As an exercise we calculated the total ozone climatology diagnostic over the SH only and reported the results in the revised manuscript. The modification resulted in models ranking changes by 0-1 positions.

14. "Section 4.1 Total ozone p. 19359, l. 20: add the model names after 'observed trend' and 'factor 2'"

Done

15. "l. 24: Cite Eyring et al. after 'ozone trends'"

Done

16. "l. 24-28: UMETRAC has high grades for polar Cly, but not for mid-latitude Cly in WE08."

We specified that our discussion is related to Antarctic ozone and Antarctic Cly.

17. "It would be relatively simple to look at the Cly trends in the new SOCOL simulation, rather than keeping a vague statement 'this maybe somewhat different'."

Thanks for this comment. We looked at the Cly trends in the new SOCOL simulations and changed the discussion as follows: 'Note that in the newer version of SOCOL used here the simulated Antarctic Cly values are considerably closer to the observed ones than those used by WE08 but still remain smaller than those in observations and in the majority of CCMVal-1 models (not shown).'

18. "p. 19360, l. 1: 'TOMS/SBUV errors' should be replaced by 'TOMS/SBUV biases

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compared to the Rosenlof data set'."

Corrected

19. "l. 28 and entire text: could you please use proper wording and refer to the 'Randel and Wu data set' rather than saying 'Randel has typically larger values', 'Randel comprises only data from the Syowa station' etc?"

Corrected

20. "Section 4.3 Combined errors This section needs improvement. Without any information the authors show 50 hPa ozone profile climatology and trend in the lower panel of Figure 11; this is inconsistent with Table 2 where these diagnostics are given for different levels."

Following the other suggestions of the Reviewer (see below) we change the discussion in this section. In particular, revised Figure 11 consists of only one panel (original panel (a)). The lower panel of Figure 11 (panel (b)) is excluded.

21. "Figure 11: The authors went through the effort of using 2-3 observational datasets in the grading exercise and now the summary figure is presented without error bars. There are huge differences in the observational datasets for the vertical ozone distribution climatology (Figure 6) and trends (Figure 8) but this is not reflected in Figure 11. It might be worth showing the grades calculated for all three observations individually in addition to the combined observed data sets. If the authors are concerned about the numbers of figures, Figures 7 and 10 to my mind are not really needed. This figure is also to small."

Thanks for these suggestions. In revised Figure 11 we demonstrate performance indices calculated using the individual reference data sets separately. Please note that the performance index is calculated from four individual diagnostics. For the Randel and Hassler data sets we do not have corresponding total ozone data sets and therefore use the TOMS/SBUV data set to calculate the total ozone diagnostics. The applied

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sensitivity tests suggest that the model ranking may change by 1-2 positions. We chose to keep Figs. 7 and 10 since they illustrate how the model errors change depending on the reference data set chosen.

22. "Section 4.4 Alternative grading Page 19363, line 8: I don't understand the logic why a different grading metric is used here. This section could be improved by (a) repeating the grading in Section 4.1 to 4.3 with the metric defined by WE08 to discuss the sensitivity of the ranking to the choice of the metric (b) use the same metric as defined in Eqs. 1-4 and apply it to the new and additional diagnostics (c) use the metric of WE08 in addition to discuss sensitivity for the ranking of the new diagnostics."

Thanks for these suggestions. Following the reviewer suggestions we change the discussion in the revised version and apply the metric defined by WE08 to the original diagnostics (i.e. following the suggestion (a)), instead of applying it to a new set of diagnostics, as was done in the original version. Please note that, considering only one variable (ozone) and concentrating on a specific region (SH high latitudes); it is difficult to design additional diagnostics that would be both sensible and independent from the original diagnostics. Therefore we do not implement the suggestions (b) and (c). We also removed the discussion of the simplified diagnostics since, as suggested by Dr. Thomas Reichler, they maybe not a good metric. Instead we assess ranking uncertainties by modifying the original diagnostics, considering independent model simulations, and evaluating the model errors with respect to individual reference data sets separately. These results are discussed in Section 4.3 of the revised manuscript.

23. "p. 19365, l. 19-29: it is not a surprise that if the focus here is on polar ozone that the correlation to the WE08 grading is not good. Please reword. It is clear that the key processes that determine ozone vary between regions and altitudes."

Please note that we also compared our grading with the polar dynamics diagnostics of WE08 and found rather modest correlation, as reported in Section 4.4. We reworded the last two sentences of this paragraph as follows: 'The differences between

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our grades may be explained by, first, taking into account that some important diagnostics (e.g. those related to polar chemistry) may be not considered in their study, and second, that those diagnostics that were considered may need to be given different weights depending on their importance for polar ozone.'

24. "p. 26: remove the personal communication and base this statement on Eyring et al. 2006 and WE08. WACCM gets low grades in the zonal mean wind diagnostic since the vortex breaks up too late (see Figure 2 of Eyring et al 2006) but it does perform better in Cly and other polar diagnostics."

We placed the reference to Eyring et al (2006) and WE08 and reworded the sentence so that it is clear that the overall low grade of WACCM in the polar diagnostics is because of its low grade in the wind diagnostic: 'In the case of polar dynamics diagnostics the most considerable difference is that WACCM which has the highest performance index among the individual models in our analysis gets low grade in the polar dynamics diagnostics BECAUSE OF LOW GRADE in the zonal wind diagnostic (Eyring et al., 2006; WE08).'

25. "Section 5 Conclusions p. 19367, l. 6: Eyring et al. (2008) should be (2007)"

Corrected. We referred to Eyring et al. (2006) here.

26. "l. 10.13: this discussion is again misleading. It is clear that if the focus here is on polar ozone a comparison to the grading in WE08 for all processes, including those in the tropics, will not lead good correlation."

Please note that we also compared our grading with the polar dynamics diagnostics of WE08 and found rather modest correlation, as reported in Section 4.4. We reworded the discussion as follows: 'However, comparison with the other model ranking based on evaluation of dynamics and transport in the model showed only modest correlation, probably because some processes important for polar ozone, which is given large weight in our study, were not considered by WE08, or because those diagnostics that

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were considered may need to be given different weights depending on their importance for polar ozone.'

27. "l. 13: 'likely' can be removed; it is crystal clear that the importance of processes depends on the region."

Corrected

28. "l. 15-18: There is no logic that the second sentence follows with 'therefore' from the first one."

Thank you for pointing this out. A similar comment was made by the other reviewer (T. Reichler). Please see our detailed response to him.

29. "Figures Figures 1-10: can the individual panels (in particular the width) be increased?"

We increased the width of the individual panels.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19351, 2009.

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