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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General Comments:

This paper uses a set of diagnostics and performance metrics to quantify the ability of chemistry-climate models to simulate ozone in the last two decades of the 20the century. The paper adds new and important information to the discussion of performance metrics in (chemistry-) climate modeling. Rather than a previous related study by Waugh and Eyring, 2008 (hereafter WE08) which focused on the evaluation and grading of processes that determine stratospheric ozone, this study evaluates stratospheric ozone itself. It builds on previous work, but extends it in several important aspects. The study aims to provide information on the best available future ozone dataset that can be prescribed in climate models without interactive chemistry. The authors conclude that the multi-model average ozone projection is the best estimate of

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future ozone evolution.

The paper is generally well-written and well-structured. However, in several places the presentation and analysis can be improved and some of the conclusions are too general. So some aspects of the paper should be subject to further changes, see detailed comments (mostly minor) below. After these comments have been taken into account, I can recommend publication in ACP.

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

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.

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

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.

Section 2 Data

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In previous CCMVal-1 studies 13 CCMs participated; why are only 12 CCMs used here?

Section 3 Method

Change 'Data set' to 'observations' in line 26 on page 19355 and line 7 on page 19356

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

line 6/7: 1980-1984 should not be called a pre-ozone hole climatology since ozone loss has already started (see you Figure 1)

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

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); Can the uncertainty in the observed variable (sigma) be added as an additional column?

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.

Section 4.1 Total ozone

p. 19359, l. 20: add the model names after 'observed trend' and 'factor 2'

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

l. 24-28: UMETRAC has high grades for polar Cly, but not for mid-latitude Cly in WE08. It would be relatively simple to look at the Cly trends in the new SOCOL simulation,

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rather than keeping a vague statement 'this maybe somewhat different'.

p. 19360, l. 1: 'TOMS/SBUV errors' should be replaced by 'TOMS/SBUV biases compared to the Rosenlof data set'.

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?

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.

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.

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.

p. 19365, l. 19-29: it is not a surprise that if the focus here is on polar ozone that the

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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.

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.

Section 5 Conclusions

p. 19367, l. 6: Eyring et al. (2008) should be (2007)

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.

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

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

Figures

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

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