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Interactive comment on "Quantitative evaluation of ozone and selected climate parameters in a set of EMAC simulations" *by* M. Righi et al.

Anonymous Referee #1

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The manuscript presents an evaluation of the ECHAM/MESSy Atmospheric Chemistry (EMAC) model under four different configurations, including two simulations where the dynamical fields are nudged towards reanalysis and two free runs. The evaluation is performed through the Earth System Model Validation Tool (ESMValTool), comparing model fields against a database of physical climate and chemical observations built into the ESMValTool. For physical climate variables, the comparison of the free-running simulations with observations (largely reanalysis data) shows the EMAC has biases that are comparable with other state-of-the-art climate and chemistry-climate models, including a cold bias in the lowermost extra-tropical stratosphere and a too-weak Antarctic polar vortex. For chemical variables, the stratospheric temperature biases over Antarctica lead to a significant underestimation of ozone depletion in Austral spring. While tropospheric ozone is found to be biased high in general, except for a

simulation with very low lightning NOx emissions. Additionally, a pair of sensitivity simulations are presented to investigate the possible importance of two poorly understood chemical processes.

The paper presents a fairly comprehensive evaluation of the present-day physical and chemical climatology of the EMAC model, with a particular emphasis on exploring the sensitivity of the simulations to nudging of the dynamical fields towards reanalysis. Overall, I find the material to be well presented and I have only a few concerns on particular aspects of the paper that should be addressed.

Frequently through the paper the differences between the two simulations that are nudged towards the reanalysis are qualified by stating that the nudging is only weakly applied. The paper quotes e-folding times of 12 hours for temperature and surface pressure, 6 hours for vorticity and 48 hours for divergence. It is also pointed out in the paper that the full strength of the nudging is only applied between approximately 200 hPa and 700 hPa, but at these levels a 12-hour e-folding time for temperature nudging is quite strong. Widely used e-folding times for dynamical fields I have seen are somewhere around 24-hours, leading me to think that the nudging is actually quite strong – with the caveat that there is no nudging in the stratosphere. Is it possible to include a bit more background information, perhaps from the Jockel et al. (2006) paper, on why the two nudged simulations analysed here are only weakly nudged?

The concern about how strongly the nudging is applied leads directly to the second point, which is the existence of the significant lower-stratospheric temperature bias in the nudged runs. In Figure 1 the tropics at 200 hPa show a larger temperature bias in the nudged runs than in the freely running simulations, though this is compensated for by a larger cold bias in the free runs in the extra-tropics so that the global-average bias is very similar in both the freely-running and nudged simulations. If the temperatures are being nudged towards the operational ECMWF analysis with a time constant of 12 hours, how is a global-average 5 K temperature bias supported in the nudged runs? I'll note that the ECMWF operational re-analysis is being used for the nudging, while validation is against the ERA-Interim reanalysis but I am guessing there is not a 5K difference in these datasets at 200 hPa.

The other significant point is about the effects of sea-surface temperatures and sea-ice on Antarctic ozone depletion, discussed in section 6.2.1. Here two freely-running simulations are compared, the TS2000 and ACCMIP runs, that used different SSTs and the conclusion is drawn that the particular set of SSTs used in the ACCMIP simulation has contributed to the deeper ozone depletion found in this simulation as compared with the TS2000 simulation. There have been some results reported in the literature that show connections between surface processes and the evolution of the Antarctic stratospheric vortex (e.g. Garfinkel et al., J. Atmos. Sci., 70, 2137-2151, 2013) so it seems reasonable to expect a connection. My concern is whether the differences between the TS2000 and ACCMIP simulations are statistically significant. I am also concerned about the extension of the findings from the two free-running simulations to the nudged simulations. In section 6.2.1 it is stated that the ECMWF data used to supply SSTs for the nudged simulations also seems to favour a deeper ozone depletion, yet these simulations used nudging of dynamical variables. The connection between planetery waves and the polar vortex is well known and in the two nudged simulations the planetary waves will be significantly influenced by the nudging, so I think it is a point for further analysis whether the nudging or the SSTs can explain the greater ozone depletion in the two nudged simulations, EVAL2 and QCTM. There is also the added complexity that the QCTM simulation has specified ozone fields that interact with the model radiation and will then feed through to the evolution of the polar vortex. To sum it up, there is an interesting case to be made if the two freely-running simulations do demonstrate a statistically different amount of ozone depletion, but the argument about whether the ECMWF SSTs used for the nudged simulations also favour greater ozone depletion should be approached with a much greater degree of caution.

Additional, more minor comments are given below.

Page 6551, Lines 9-10: The reference to the exact bias being referred to by '... sign-

ficantly reduces this bias.' is the overestimation of tropospheric ozone but it was a bit unclear on first reading since it is a long passage.

Page 6557, Lines 14-20: In the description of the QCTM experiment, is the nudging setup in an identical manner to that used for the EVAL2 experiment?

Page 6558, Lines 1-2: It it stated that the TS2000 experiment uses the same emissions setup as for the QCTM experiment, but looking into Table S1 it seems there are a few minor differences for emission categories such as biomass burning and land transport.

Page 6559, Lines 21-28: The discussion of the CMOR standard seems to confuse the CMOR software tool with the Climate and Forecast (CF) standard. The CMOR is a software library that is designed to write out netCDF files that comply with the CF conventions, but is not a set of standards itself. You can have a look at http://cfconventions.org/

Page 6565, Line 2: There is a '300' that should be '30'.

Page 6565, discussion across lines 21-28: It is interesting to note that the QCTM simulation has a global average temperature at 30 hPa that is quite different from the other simulations. Since the QCTM run uses prescribed ozone and water vapour for the model radiation, this might be a sign of the impacts of biases and interactions between chemistry and radiation.

Page 6566, lines 15-29: The discussion of lowermost stratospheric temperature bias in the extratropics is linked to a high bias in water vapour as compared to the HALOE observations, as shown in Figure 3. While water vapour certainly does seem a bit high in the simulations, it is worthwhile noting that HALOE is believed to be biased low in this region. See the results of the SPARC water vapour assessment published in Hegglin et al., J. Geophys. Res., 118, 11,824–11,846, doi:10.1002/jgrd.50752, in particular their Figure 9 which compares HALOE with other satellites at 150 hPa, noting also that it is believed that the HALOE bias increases quite rapidly below this level.

Page 6567, Lines 11-14: Here it is stated that it is not surprising that the EVAL2 and

QCTM nudged simulations reproduce the observed absolute values and annual cycle in 100 hPa zonal average tropical temperatures better than the free runs, but these two simulations had a considerably worse comparison with observations for 200 hPa tropical temperatures as shown in Figure 1. It would not seem to be a straight-forward result of nudging, particularly considering that the nudging is applied less strongly at 100 hPa.

Page 6568, Line 19: The figure showing the eastward wind (S3) is put into the supplementary material, but there is considerable discussion of this figure in the text of the article. Can I suggest moving S3 into the main article? Note also that the caption on Figure S4 references DJF mean, but I think it should be JJA mean.

Page 6570, Lines 26-28: Here the strong annual cycle in specific humidity is attributed to the annual cycle in incoming solar radiation that affects evaporation. There must also be a role for the annual cycle in air temperature, which controls how much water vapour the air can hold?

Page 6571, Line 9 – Page 6572, Line 8: Here reference is made to figures S-11 through S-13. Is it possible, and not too much work, to annotate the figures with the global average values for these fields? These can quite helpful for the radiation budget terms.

Page 6575, Line 2: The sensitivity of the tropospheric ozone column to the tropopause definition is always a problem. But I do want to point out that the reference to Table 3 in Stevenson et al., 2013 is not exactly correct in that Table 3 presents the sensitivity of the 1850 to 2000 change in tropospheric ozone column and not the sensitivity of the absolute ozone column. The 1850 to 2000 in EMAC does seem to be more sensitive than for other models, but it is not clear that this sensitivity also applies to the absolute amounts for a particular time period.

Page 6575, Lines 16-21: The discussion of the lightning NOx emissions focuses on the differences between QCTM and EVAL2, but then this made me wonder about the lightning in the other two simulations that also had a high bias in tropospheric ozone.

It is shown in Table S2, but probably worth mentioning here that TS2000 and ACCMIP use a similar 11 to 12 Tg-NO/year for lightning NOx.

Page 6576, Lines 21-26: I find it noteworthy that the annual cycle in ozone is pretty well reproduced by the model for all the regions shown in Figure 15, except for the tropics at 500 and 250 hPa.

Page 6576, Lines 16-26: The argument that CO could be used as a helpful, indirect indicator of global-average OH seems to be a bit weak. I certainly agree that CO is an important species for tropospheric chemistry and should be assessed, but global-average OH is much more tightly constrained by methylchloroform decay. Given uncertainties about CO emissions and CO sources from hydrocarbon oxidation, I cannot imagine any constraints on OH through CO being as stringent as that found from methylchloroform. Prather et al. (Geophys. Res. Lett., 39, doi:10.1029/2012GL051440, 2012) argue that global-average OH is constrained to about +/- 12% from methylchloroform.

Page 6582, Lines 11-16: I quite like the argument of how the addition of the HNO3-forming channel for NO + HO2 has impacted the distribution of CO. The increase in CO has come, I assume, from decreases in OH and so it seems the argument becomes a bit circular when it is said that the increased CO could lead to decreased OH. Can I suggest the slightly different viewpoint that the new steady-state for CO is the result of changes in OH induced by the addition of the NO+HO2 channel, along with the positive feedbacks of increased CO further reducing OH. In the end, perhaps it is nothing more than a change in wording, but it seems to me to be a bit clearer representation of how tightly coupled the system is.

Interactive comment on Geosci. Model Dev. Discuss., 7, 6549, 2014.