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Interactive comment on "Evaluation of the new UKCA climate-composition model – Part 2: The Troposphere" *by* F. M. O'Connor et al.

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The authors would like to thank Reviewer 1 for a comprehensive review of the original submitted manuscript. Below is a detailed response to the comments raised.

Comment: The manuscript details one variant of the UK Chemistry and Aerosols (UKCA) model; presented here is the StdTrop configuration that has been used for CMIP5. The significant components of the model are described and a wide ranging series of model results are presented and compared with observations, from radionuclides to assess transport and deposition, to chemical fields for methane, ozone, CO and NOx. While the comparison with observations is broad and generally well presented, there seem to be a number of important comparisons that are omitted from the article, and which are frequently included in similar model description articles, leaving

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the reader with an incomplete view of the model. The assessment of the model carbon monoxide, presented in Section 5.2, is an example. The comparison of surface CO to a selection of available observation stations is routinely done for these type of model description papers and is well presented here. To investigate the distribution of CO away from the surface, comparisons are made with observations from a variety of aircraft campaigns performed during the 1990s. From the comparison, the model is found to be biased high or low depending on the field campaign and the comparison as a whole is subject to the caveat that '. . . the comparison at best is expected to be qualitative.' There are really fantastic satellite observations of CO available and the reader is referred to comparisons with TES to be found in Voulgarakis et al (2011b) and Telford et al. (2013) but it is not readily apparent how applicable these comparisons with TES are since they were conducted with the TropIsop version of the model. Since this manuscript is meant to provide the reference for the StdTrop version of UKCA and the comparison with the aircraft observations seems inconclusive, I would strongly suggest replacing the comparison with aircraft observations presented in Section 5.2 with a comparison of CO with satellites observations.

Reply: The authors accept these comments from Reviewer 1. As a result, they plan to run a parallel simulation in which the climate model is nudged to ECMWF analyses (Telford et al., 2008), thus enabling a more quantitative comparison of modelled O3 and CO with TES observations to be carried out (e.g. Voulgarakis et al., 2011). Such a comparison will be included in a revised manuscript.

Comment: The distribution of ozone for January shown in Figure 22 shows concentrations over northern hemisphere continents almost uniformly below 20 ppbv. Very low concentrations can also be seen near the surface in the northern hemisphere in the cross-section of zonally averaged ozone in Figure 23. Looking back to Hauglestaine et al. (2004), January surface concentrations are only very rarely below 20 ppbv over northern hemisphere continental regions, with large areas showing average values of 25 – 35 ppbv. A similar range of values is shown by Zeng et al. (2008) for January

over northern hemisphere continental locations. I would suggest the inclusion of a few continental sites for surface ozone observations. Many of the sites presently used for the comparison are island or coastal sites and while Niwot Ridge is continental, it is also at high altitude. Other papers have frequently compared to Hohenpeissenberg and find pretty good comparisons, so that would be one suggestion. Is there an idea why the UKCA model produces such low values over continental regions?

Reply: The authors accept that wintertime northern hemisphere surface ozone is possibly too low relative to other models and observations. One possible cause is excessive titration by NO although it may also be related to the boundary layer mixing scheme. This will be investigated further and an appropriate discussion included in the revised manuscript.

Comment: The absence of a global ozone budget for the model is also of note. Although largely theoretical, an ozone budget is frequently a component of these model development papers and allows the reader to ascertain the size of strat-trop exchange and dry deposition.

Reply: The global tropospheric ozone budget from the model, based on the 10-year simulation described in the original manuscript, is as follows:

Dry Deposition = 931 +/- 4 Tg O3/year; Chem. production = 3064 +/- 18 Tg O3/year; Chemical loss = 2736 +/- 19 Tg O3/year, and Stratospheric influx (inferred) = 603 +/- 11 Tg O3/year

This budget, in the context of the tropospheric ozone budget from other models (e.g. Stevenson et al., 2006; Young et al., 2013) will be included in a revised version of the manuscript.

Comment: While the comparison with the aircraft campaign CO was inconclusive, these campaigns also provide some of the best (only??) observations for reactive nitrogen compounds like HNO3 and PAN. I note there is a comparison for NOx in the

paper now, but nothing similar for HNO3 and PAN. Why have these been omitted? Perhaps PAN will be significantly underestimated without isoprene chemistry, but that fact would be important to establish - particularly in relation to the other articles on different versions of UKCA.

Reply: These comparisons had been performed but were omitted from the original manuscript due to space. In light of Reviewer1's comments, these can be included in the revised manuscript. As suggested without isoprene chemistry, modelled PAN is on whole biased low in comparison with the observations.

Comment: Page 1749, Lines 1-5; Is the aerodynamic resistance (r_a) tied to the model treatment of the surface layer? A sentence or two on how r_a is derived would help complete the description.

Reply: The aerodynamic resistance ra is calculated from the wind profile taking into account atmospheric stability and the surface roughness:

ra = (ln(z/z0) - Ψ) / (ku*)

where z0 is the roughness length, Ψ is the Businger dimensionless stability function, k is von Karman's constant, and u* is the friction velocity. This can be included in the revised manuscript to provide the description completeness that Reviewer 1 is requesting.

Comment: Page 1752, Line 27; Figure 3 shows the model values with a red vertical line. I assume it represents a moment of the distribution of the model values, but what exactly should be specified.

Reply: Figure 3 shows a comparison between modelled and observed multi-year monthly means. The vertical lines then indicate show the multi-annual monthly mean plus and minus one standard deviation about the mean. This is clarified in the figure's caption in the revised manuscript.

Comment: Page 1757, Lines 16-20; Has the mass conservation also been assessed

by comparing the 210Pb deposition (for the tracer that is deposited) against the 222Rn emission? Analysing the non-deposited 210Pb would capture conservation errors in the 222Rn, but the non-deposited 210Pb would very quickly become quite homogeneous on at least the hemispheric scale.

Reply: Mass conservation hasn't been assessed by comparing the Pb deposition with the Rn emissions. This could be quite an interesting alternative test and will be included in the revised manuscript.

Comment: Page 1760, Line 3; Where it is stated that 't_d is the e-folding lifetime', does that refer to the lifetime to radioactive decay of 10.76 years given a bit earlier? As it is currently written, the terminology is a bit inconsistent.

Reply: The 10.76 years quoted is the half-life, not the e-folding lifetime. To make it consistent with t_d, the value for the e-folding lifetime of 15.52 years has replaced the half-life value.

Comment: Page 1767, Line 24; I don't see the need to include Figure 12, showing the spatial distribution of CO emissions.

Reply: Figure removed, as suggested.

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