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Geoscientific Model Development Discussions

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

Anonymous Referee #2

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This paper describes the UKCA atmospheric chemistry-climate coupled model, and presents an evaluation of the global tropospheric distributions and abundances of several key trace gases simulated by the model. The paper is very well written, and is a thorough description of the important aspects of the model gas phase chemistry, and serves as a useful 'benchmark' for the evaluation of this community model. The authors have gone to much effort to include detailed comparisons with observations where appropriate, in addition to an extensive comparison with previous evaluations of similar models. The paper is wholly appropriate for publication in GMD, and I recommend that it is accepted for final publication following modifications to address the points I raise below.

My main concern with the paper is the presentation of the StdTrop chemistry scheme

as the 'default' model scheme, and a lack of direct comparison with the results of a simulation using the TropIsop scheme. While I understand that the StdTrop scheme has been used in the HadGEM simulations for CMIP5, the lack of higher VOC chemistry (most notably, isoprene) means that it probably leads to biases compared with the schemes used in most current-generation CTMs and ESMs. It would be useful to document here some of these biases that are to be expected when using StdTrop. This could be presented using direct comparisons here, or by more explicitly referring to the appropriate diagnostics from another study in which TropIsop is used in the same model configuration. Of particular interest are differences in methane lifetime, ozone burden, and the spatial distributions of ozone and NOy. In particular, I would expect a lack of isoprene to lead to large differences in NOy partitioning (due to large reduction in PAN formation), with consequent impacts on the tropospheric ozone distribution. In addition, As a reader of this paper, it would be useful to know how large an impact such differences may have. e.g. in Figure 26, it would be useful to know how different the mean ozone bias is for this simulation vs a similar simulation with TropIsop.

Throughout the paper there are several places where comparisons with observations are described with insufficient quantitative information. e.g. phrases such as "model performs well", "in excellent agreement", "quite well", "very good", etc. are all overly subjective for a scientific paper. Please, where possible, quote mean bias or RMSE values when discussing comparisons. This has been done already in some sections (e.g. discussion of ozone biases on Page 1785).

Specific comments:

Page 1747, line 19: Please give approximate horizontal resolution corresponding to N96, and the pressure range over which 38 levels are spaced.

Page 1749: Dry deposition scheme. What is used to map out the 9 surface types considered by the dry deposition scheme? Presumably these are prescribed from somewhere in the absence of an online vegetation simulation.

Page 1751, line 8: "inert species" suggests zero loss rate. Suggest change to "fixed lifetime species" or "prescribed lifetime species".

Page 1752, line 3: ".. allowed to spin up". How long was used for the spin-up period?

Page 1758, line 17: 85Kr simulation was started on 1 September 1978. What does this date refer to? Is this a simulation nudged to real meteorology, or does this date refer to the emissions data? Please clarify.

Page 1762, line 15: "StdTrop scheme is more representative of the background troposphere.." Please justify this (see also my main concern above). Does this suggest that biases between model and observations in the remote troposphere are very similar for the StdTrop and TropIsop schemes (and more similar than in polluted continental regions). It would be useful to show that this is the case if the authors wish to use this to justify the inclusion of the StdTrop scheme as the default. Previous studies have shown the chemistry of the background troposphere under clean (e.g. pre-industrial) conditions to show large sensitivity to isoprene emissions (e.g. Mickley et al., 2001).

Page 1765 & Figure 11: Discussion of photolysis schemes. Again, it would be useful here to point out the extent to which the choice of photolysis schemes affects key parameters such as global mean [OH], zonal mean OH and ozone distributions.

Page 1770/1771: Methane evaluation. With a 10-year simulation, but fully explicit (i.e. emitted and OH oxidised) methane treatment, to what extent is the global methane distribution dependent on the initial condition? Is a 10-year simulation long enough to evaluate the model-simulated methane distribution?

Equation numbers appear to be absent. Not sure if this is journal style, or if they are missing.

Finally, a question on model and scheme names: Is it appropriate to refer to the model throughout the paper as UKCA, since (as far as I understand) this name is used to refer to the chemistry/aerosol scheme, not the model as a whole, which here is HadGEM2?

Maybe this needs to be clarified.

References

Mickley, L. J., D. J. Jacob, and D. Rind, Uncertainty in preindustrial abundance of tropospheric ozone: Implications for radiative forcing calculations, J. Geophys. Res., 106, 3389-3399, 2001.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1743, 2013.