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Interactive Comment

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 2 for a comprehensive review of the original submitted manuscript. Below is a detailed response to the comments raised.

Comment: 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,

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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.

Reply: In addressing Reviewer 1's comments on doing a quantitative comparison with TES satellite observations, a nudged model simulation with StdTrop will be carried out. This can also be compared with nudged simulations of the TropIsop scheme (included in Voulgarakis et al., 2011) to address your main concern with the manuscript as it stands. Although there will be differences in the simulations as regards resolution, chemical solver, and climate model configuration, the nudged simulations with StdTrop and TropIsop will have used identical meteorology and consistent emissions and could be used to establish key differences as a result of the chemistry. A discussion on these differences (including references to appropriate papers) will be included in the revised manuscript.

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Comment: 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).

Reply: Quantitative comparisons were intentionally not carried out for the aircraft measurements because the model isn't running with specified dynamics and the measurements themselves are restricted to certain time periods. However, this isn't the case with the surface observations and a more quantitative assessment would be more objective. For example, model scores, absolute annual mean bias, relative annual mean bias, correlation co-efficients will be included in the revised manuscript. In addition, following a suggestion from Reviewer1, a nudged model simulation will be carried out to compare with CO and O3 TES observations. Again, a quantitative rather than a qualitative assessment from this additional comparison will be included in the revised manuscript.

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

Reply: N96 corresponds to a horizontal resolution of $1.875^\circ \times 1.25^\circ$, which equates to about 140 km at mid-latitudes. The 38 vertical levels are hybrid height levels with the model lid at approximately 39 km. The thickness of a model level at the tropopause, for example, is of the order of 1 km (Fig. 2b in the paper by The HadGEM2 Development Team: Martin et al., 2011). This text can be added in the description of the model configurations.

Comment: 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.

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Reply: There are 9 surface types considered; these are broadleaved trees, needleleaf trees, C3 and C4 grass, shrub, urban, water, bare soil, and land ice. In the case of simulations with non-interactive vegetation, these are prescribed using distributions from the IGBP (International Geosphere-Biosphere Programme). This can be included in the revised manuscript to provide greater clarification.

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

Reply: Done.

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

Reply: The spin-up was for 4 months. This has now been included in the revised manuscript.

Comment: 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.

Reply: The model itself was free-running and is not using specified dynamics. However, it is using sea surface temperatures, sea ice distributions, and prescribed emissions appropriate for the time period. The initialised modelled Kr fields are also appropriate for the time period and were taken from Rind and Lerner (1996). This will be clarified in the revised manuscript.

Comment: 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

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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).

Reply: The authors accept that this is a valid point and a comparison between StdTrop and TropIsop will help highlight key differences in the remote troposphere caused by the inclusion of an isoprene mechanism. The text will be altered accordingly in the revised manuscript.

Comment: 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.

Reply: Telford et al. (2013) (http://www.geosci-model-dev.net/6/161/2013/gmd-6-161-2013.html) already detail some of the key differences in model performance between the offline photolysis and Fast-JX. Some additions to the revised manuscript could assess the impact of the offline photolysis versus Fast-J, thereby covering the impact of all 3 photolysis options.

Comment: 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?

Reply: The authors accept the comment that a 10-year simulation alone could potentially be sensitive to initial conditions. However, the model had been spun up prior to the 10-year simulation and the modelled burden and budgets during this spin-up were checked to ensure that the modelled methane field was in steady state. For the 10-year simulation analysed here, the methane burden and budget for the first 5 years were compared against those from the second 5 years; no significant difference was found. Likewise, the choice of years made little difference to the comparison against observations. A statement has been added to the revised manuscript to say that the

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model had been sufficiently spun-up prior to running the 10-year simulation.

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

Reply: They are missing. Now added to revised manuscript.

Comment: 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.

Reply: The name UKCA does indeed refer to only the chemistry and aerosol component, which is coupled to different climate model configuration e.g. HadGEM2, HadGEM3. The revised manuscript will refer to the model as the "coupled HadGEM2-UKCA model" for clarity.

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