

Interactive comment on “An improved parameterisation of ozone dry deposition to the ocean and its impact in a global climate-chemistry model” by Ashok K. Luhar et al.

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We thank the Referee for reviewing the manuscript and providing very useful comments. In the following, we provide a response to the comments made by the Referee (the Referees' comments are shown in inverted commas).

Comment: “. . . The paper addresses a neglected but important topic and covers a relevant subject for this journal. Overall, the paper reads well and should be published, after addressing the specific and technical comments below. These are mostly minor or suggestions to improve clarity.”

Response: Thank you for your comments.

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Comment: “Specific Comments:” “Deposition Schemes: The ‘one-layer’ scheme is effectively that developed by Fairall et al. (2007). The ‘two-layer’ scheme includes a surface microlayer layer and some pragmatic choices are made to make it computationally tractable. The description of the schemes includes a significant number of equations, which for the one-layer scheme repeat to some extent those given in Fairall et al. (2007). As a suggestion, the authors may wish to consider moving these to an appendix or supplement to aid clarity.”

Response: We appreciate the suggestion by the referee, but we think that the number of equations presented in the paper provide a good, self-contained description of the deposition schemes used with logical flow, moving from the simple default scheme to the two-layer reactivity scheme. In doing so, obviously, there is some overlap with relevant equations from the paper by Fairall et al. (2007). We think that keeping the equations in the main text is helpful because they are frequently referred to in the main text when discussing the model intercomparison results and interpreting the model behaviour against the data.

Comment: “Iodide fields: Based on earlier experimental studies, the O₃ deposition process includes inter alia a reaction of O₃ with iodide (I⁻) in the surface layers of the ocean (page 7, line15). Initially, I could not see the relevance of the subsequent chemistry (equations 6 to 10 on page 8), except perhaps to indicate that the iodide is effectively regenerated (or how I₂ is formed). Are these equations needed?”

Response: The referee is correct. The purpose of presenting Equations 6 to 10 on Page 8 was to highlight the complexity of the ozone-iodide chemistry, to indicate how I₂ is generated, and to suggest that there could be a reverse reaction freeing the bound ozone (see bottom of Page 16 and top of Page 17). We will delete these equations, add a sentence and point to the appropriate references cited.

Comment: “The cited paper by Ganzeveld et al. (2009) used sea-water nitrate as a proxy to derive global iodide fields. In this study, sea-surface temperatures are used

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(after Sherwen et al.). Later in the paper (page 18, line 9 and Figure 11), a qualitative comparison is made with the modelled O₃ deposition velocity maps in Ganzeveld et al. (2009). There were latitudinal differences in the position of the maxima. One can arguably see the same mid-latitude Atlantic feature that Ganzeveld et al. reported (for July 2006). Allowing for the other differences mentioned (page 18, line 15), it would have been interesting to see a simulation using an iodide field based on nitrate.”

Response: In our work, the oceanic iodide concentration was parameterised in terms of sea-surface temperature (SST) following MacDonald et al. (2014). A sensitivity analysis was also performed using the parameterisation by Chance et al. (2014) which also used SST (see Page 15). As mentioned on Page 10, a comprehensive analysis of available iodide measurements conducted by Chance et al. (2014) showed highest iodide concentrations in tropical waters. They examined statistical relationships between iodide and parameters such as SST, nitrate, salinity, chlorophyll-a and mixed layer depth, and found that SST was the strongest predictor of iodide followed by latitude. The correlation (r^2) of iodide with SST² was 0.61 while that with nitrate was 0.36. The parameterisation by MacDonald et al. (2014) is very similar to that by Chance et al. (2014) in that it also involves SST but yields lower iodide magnitudes. Hence the data indicate that the use of SST is preferable to nitrate.

Comment: “Nudging and monthly-averaged fields: The model is nudged to ERA-Interim reanalyses of wind and temperature. Thus, synoptic variability is similar to that observed, improving the comparison with observations of atmospheric trace constituents. Monthly-averaged concentration fields are used in the analysis reported here. To those more familiar with land-based deposition, the use of monthly-averaged fields would not seem realistic, given the strong diurnal variations in O₃ concentrations and in the stomatal uptake over vegetation-cover surfaces. It might be worth contrasting this with the behaviour over oceans where the diurnal variations are expected to be smaller (presumably), which justifies the use of monthly-averaged fields. What about coastal regions, which are more influenced by the land???”

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Response: As stated in the 2nd last paragraph on Page 11 of the paper, oceanic diurnal variations relevant for ozone dry deposition, especially of SST which is a key parameter, are relatively small (also see below the next comment) so the use of monthly-averaged fields can be justified. We will expand the text on Page 11 to highlight the fact that there is a strong diurnal variation of land-based deposition in contrast to the weak diurnal dependence of oceanic deposition.

With regards to coastal grid cells, these cover a fraction of the water tile and fractions of other surface tiles. As described in the 2nd last paragraph on Page 6, for a particular grid cell the resistances and the corresponding deposition velocities are calculated for all surface tiles that constitute the overall surface type of that grid cell. A first order loss rate is calculated corresponding to the deposition velocity for each tile, and a grid-cell mean loss rate is obtained by weighting the individual loss rates with the fractions of the surface tile types present in the grid cell. In our analysis for the oceanic deposition, we do not consider any coastal grid cells and only consider those grid cells that are 100% covered by the water tile. We will state this explicitly in the paper.

Comment: “The UKCA atmospheric chemistry module used in ACCESS has a branch, which includes an aircraft/satellite emulator, allowing the model to be sampled at the time and location of an aircraft or satellite sounding (http://www.ukca.ac.uk/wiki/index.php/Flight_track_emulator_to_compare_to_campaign_data). It could easily be used for ship-based measurements. The model could then be sampled at the actual time of the observation.”

Response: The flight emulator suggested by the referee is part of a UKCA development branch, and not part of any standard release at present. It would require some work on our part to incorporate it into our UKCA branch, and to adapt it to ship based cruises for the present study. However, over the ocean diurnal variations, especially of SST which is shown to be a key parameter for oceanic ozone deposition, are relatively small (which is also mentioned by the referee above and stated on Page 11 of the

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paper). Therefore, we do not expect any significant changes in our evaluation results even if an emulator was used. Moreover, we do not have information on the actual timings of the observations along the cruise tracks - only maps of cruise tracks and the number of days over which a cruise was conducted are available. The deposition velocity observations in the paper by Helmig et al. (2012) which we used are only presented as a function of SST and wind speed. We will explore the emulator option for our future model evaluation studies where appropriate.

Comment: "Performance of standard model configuration: The modelled O₃ concentrations and budgets are underpredicted. While this does not detract from the present work, it would be good to understand the source of this bias. The cited paper by Woodhouse et al. does not really provide further details as to whether this is a result of emissions, initialisation, spin-up, etc ..."

Response: The version of UKCA used in our study underpredicts the observed tropospheric ozone concentration, which is also the reason for the total annual ozone dry deposition obtained from our model to be on the lower end of the range derived based on the multi-model results given in the paper. We have not been able to pinpoint the reason for the ozone underprediction by UKCA. The ozone bias is an ongoing area of study in ACCESS-UKCA.

Comment: "Code availability Are these model developments available in an accessible code repository? If so, the authors could provide access details as I am sure that other groups would be interested in using this approach."

Response: We have not yet put these model code changes in any UM-UKCA code repository yet, but intend to do so. The formal process of transferring any model code changes to a UM trunk requires a number of steps. We intend to produce documented code and follow this process up.

Comment: "Technical comments:" "It was hard to compare Figures 5a and 6, especially on the screen. As a suggestion, I wonder if these could be combined into a single plot

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or made into a single figure on the same page to make the comparison easier."

Response: We will do that.

Comment: "The colour scale used in the panels of Figures 9 and also of Figure 11 differ. While the colour scales used aid the comparison of the spatial patterns, it makes comparison of the the absolute magnitudes harder. It might be better if a common scale could be adopted, with sufficient dynamic range and discretisation."

Response: When plotting these figures for the paper we gave some thought to the same issue raised by the referee and tried some plotting options. It was eventually a trade-off between keeping the same scale in all the plots and losing details (i.e. dynamic range). In our opinion, keeping a common scale would deteriorate the visibility of detailed gradients in the deposition velocity contours obtained using the two-layer reactivity model or would require nonlinear scaling values. Our thinking is to retain the current format of these figures.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-844, 2016.

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