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Interactive comment

Interactive comment on "Revising global ozone dry deposition estimates based on a new mechanistic parameterisation for air-sea exchange and the multi-year MACC composition reanalysis" *by* Ashok K. Luhar et al.

Anonymous Referee #2

Received and published: 14 October 2017

This paper proposes some updates to the paper the authors published earlier this year (10.5194/acp-17-3749-2017) describing the deposition of ozone to the ocean. Some changes are made to the parameterization and the resulting deposition velocities are used to explore the impacts on the global budget of ozone on the ACCESS-UK model. They have concerns about the veracity of their atmospheric chemistry model so explore the impact of the new deposition velocities with the MACC reanalysis. They conclude that their new parameterization has some skill in representing the rather sparse observational dataset and that with this new parameterization for deposition velocities, the mass of ozone deposited to the ocean is significantly reduced with implications for both the budget and distribution of ozone.

I have concerns that this paper represents a small incremental advance over the previously published paper. For example, Figure 5 only shows small difference between the new and old schemes which was published only a few months ago. Ideally this paper would have been coupled into the paper published only a few months ago. However, this is a decision to be made by the editor.

Fundamentally this paper provides a description of an improved O3 deposition parameterization for the oceans, shows that there is some fit between the observations reasonably well and fundamentally changes the tropospheric budget for ozone especially over oceanic regions. These are important conclusions.

I have a few questions and queries to suggest for the improvement and shortening of the paper which I make below. Assuming that these can be made I would recommend publication.

Major comments.

Ocean O3 lifetime.

The premise of the paper is that reaction between O3 and I- is the only sink for O3 in the ocean. There is no discussion of the validity of this assumption. There is significant evidence that dissolved organic matter (DOM) may play a significant role in deposition of ozone to the surface (see for example 10.1029/2008GB003301). Yet this isn't discussed in the text. There should be some justification given for ignoring the role of DOM in their calculations.

The parameterization appears to do a reasonable job of simulating the deposition observation (Figure 4) without the need for an additional ocean side O3 sink. However there has been a tuning of the model (top half of page 12) so it isn't obvious that a missing O3 sink process (such as that offered by DOM) would be 'diagnosed' though a model to measurement comparison. Figure 4 looks very similar to a figure shown in the author's previous paper. It would be useful to show this data in an x-y plot and give some indication of the error associated with the parameterization against the observations.

Our current understanding of DOM, its reactivity to O3 and distribution is poor. However, the authors should discuss the implications of them ignoring the potential DOM sink. Whilst they are doing that they should also discuss the implication of their choice of iodide distribution. They are using the distribution based on the parameterization of McDonald, but the literature also includes the Chance parameterization which gives higher I- concentrations and I think gives a slightly different spread. What are the implications of this?

There should be more of a discussion of the uncertainties of the O3 lifetime in the ocean, and how the parameterization tuning to the observations provides some solid ground to base the subsequent budget analysis. What impact do these uncertainties have on the budget?

Diagnosing the ozone deposition flux

The new parameterization is put into the ACCESS-UKCA model and this gives a global flux of O3 deposition to the ocean of ∼86 Tg yr-1. The model is known to have a low bias for O3 and so a significant body of work is done to calculate the flux from the MACC analysis fields of O3 and then a bias corrected MACC analysis fields. This lengthens the paper significantly for almost no gain. The canonical value for ocean deposition of O3 is around the 340 Tg yr-1 from the Hardacre study. The new parameterization gives the ACCESS model a deposition of 86 Tg yr-1, the MACC Analysis 93 Tg yr-1 and the bias corrected MACC Analysis 98.4 Tg yr-1. Compared to the Hardacre values these numbers are essentially the same (25%, 27% and 28%) respectively especially when the uncertainty in the parameterization are considered. There are pages of text describing the MACC data but I don't think it substantially changes the conclu-

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Discussion paper

sions especially as the authors are forced to bias correct the MACC data. Would it not make more sense to bias correct the ACCESS data?

My suggestion is to remove this section or to perform the bias correction on the ACCESS data. It doesn't add anything to the story but it makes the document substantially longer.

Minor Comment.

There should be more details on the performance of the ACCESS physical model. There are no details of performance, parameterization choices etc. There should be more details given. What aspects of the model impact the parameterization used?

Typo on page 10, line 14 "fullydescribe" missing a space

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