Authors' response

To gmd-2019-21 Anonymous Referee #1 (02 Apr 2019): We thank the referee for his/her useful comments and we will take them into account in the revised version of this paper. In the following, we will answer the questions in detail.

- Abstract L15-17: *"While high sensitivity to changes in dry deposition to vegetation is found in the tropics, the largest impact on global scales is associated to changes in dry deposition to the ocean and deserts."* The authors do not provide details in the paper as to what has changed in the updated scheme for such an impact.
  - Is it the surface resistance (R_c) value? Or the other two resistances (R_a and R_b)?
  - What are the typical values?
  - What value of R_c for water has been used and
  - how it compares with the value used in the Wesely scheme?

- P22 Table 3:
  - Why the deposition values for ocean + ice + land do to add up to the total values reported, for all simulations?
  - The new land-based deposition values are much lower than what has been reported in previous studies (e.g. Hardacre et al., 2015) and the authors largely attribute this to the changes in the updated scheme for the desert surface type. However, the paper does not provide any observational support to back this up. Are there any relevant deposition measurements (velocity or flux) that can be used for this purpose? At least, some comparison with ozone measurements (or even O_3 reanalyses) should be provided for this surface type (and perhaps others) to see if the model is heading in the right direction with the updated deposition scheme.
  - It will also be useful to report the global ozone burden from the various simulations.

- Section 2.1.1, Eq. (2): The statement *"For certain values of z, z0, and L, this may result in nonphysical (negative) values for Ra."* I do not comprehend as to why this would occur since this equation is simply based on the well-used Monin-Obukhov similarity theory (MOST) for the surface layer. This occurrence would also imply negative wind speeds. Actually Eq. (2) is incorrect: the term $\psi_m((z-d)/z_0)$ should be $\psi_m((z-d)/L)$, and the sign of the third term on the right-hand side should be positive (not negative). Given that $(z-d) > z_0$ (assuming the model is formulated correctly), Eq. (2) should always yield positive values. Eqs. (3–5): I am not sure why Monteith (1973) needs to be invoked here. Given that the term in the square brackets on right hand side of Eq. (2) is equal to $k \cdot u(z)/u^*$ as

per MOST, substituting this into Eq. (2) results in Eq. (5). Define z, z_0 in Eq. (2). The parameter d is the so-called displacement height, and is not a constant (depends on the surface type).

- P2 L25-33: The first reference to the Oslo CTM3 in the body of the paper is made here as *"...we have not implemented any parameterization of these processes in the Oslo CTM3 as of now."* Some brief introductory text is required here (or better at the start of the paragraph) to introduce the model properly. Also, the text between lines 25 – 33 on what is not considered in the model is too detailed to be here, so shorten and move it to Section 2. Since we intent to properly introduce our model (Oslo CTM3) in Section 2, we rather move the sentence in L30-32 about polar boundary layer ozone depletion to Section 2. Regarding that the influence of VSLS on tropospheric ozone is indirect, this reference has not been well placed in this section and will be moved to the discussions (Section 4).

- P3 L19/L28 and P21 L34: There is a newer ozone dry deposition study by Luhar et al. (2018, ACP, 18, 4329-4348) which, using global ozone reanalyses and a more realistic process-based oceanic deposition scheme, estimates the total global deposition at $722.8 \pm 87.3\,\mathrm{TgO_3 yr^{-1}}$, which includes an oceanic component of $98.4 \pm 30.0\,\mathrm{TgO_3 yr^{-1}}$. These figures should be cited for comparison. Thank you for pointing this out. We were not aware of this study and will compare our results and refer to it at the given places and within our discussion.
  - *A newer study by Luhar et al. (2018), however, indicates much lower amounts* ($722.8 \pm 87.3\,\mathrm{Tg\,a^{-1}}$).
  - *Based on the global atmospheric composition reanalysis performed in the ECWMF project Monitoring Atmospheric Composition and Climate (MACC) and a more realistic process-based oceanic deposition scheme, Luhar et al. (2018) found that the ozone dry deposition to oceans amounts to* $98.4 \pm 30.0\,\mathrm{Tg\,a^{-1}}$.
  - *But also the results of Luhar et al. (2017, 2018) yield a* $(19-27)\,\%$ *lower ozone dry deposition than the models participating in the model intercomparison, with deposition to ocean ranging between* $(12-21)\,\%$ *of the total annual ozone dry deposition.*

- Section 2.2: Since the present paper is about ozone dry deposition, this section seems like a distraction and hence should be omitted.

- Page 14, lines 15 – 18: Anthropogenic, biomass burning, and biogenic emis- sions are included in the model. How are other emissions such as soil NOx, wetland methane, and oceanic methane and CO specified?

- Page 15, line 4: The statement "Accidentally, we have used emissions for the year 2014 instead of 2005." It is not clear what the consequences on the results are of this?

- Section 3.2.1: In the Fig 5 discussion, although snow and ice is discussed, there is no discussion on the oceanic differences between the present study and Hardacre et al. (2015). This is particularly important for the Southern latitudes.

- Section 3.2.1: The Hardacre et al. (2015) simulations were for the year 2001, whereas the present study is mostly for the year 2015 emissions (see Table 1) driven by the year 2005 meteorology. In addition, the observational averages used in Fig. 8 are based on multi-year data. The authors should discuss the implications of these differences about different years on the deposition results presented (e.g. uncertainty).

- Page 24, lines 3 – 4: "The annual amount of ozone dry deposition decreases by up to 100% changing from the old dry deposition scheme to the new one." Table 3 does not support this, but this may be true for some surface types. So please qualify the statement.

- Page 24, line 15: "Most of the decrease in ozone dry deposition in the Oslo CTM3 can be attributed to changes in dry deposition velocities over the ocean and deserts." What are the dominant factors in these changes? For example, is it mostly the surface resistance (Rc) term? For the ocean, it is likely to be Rc. For deserts, maybe Rb? Is it possible to quantify these differences in the resistance terms?

- Page 24, line 24: "2-layer gas exchange with ocean waters (Luhar et al., 2017)." As mentioned earlier, Luhar et al. (2018) has derived a more realistic process-based deposition scheme for the ocean, but the results for deposition velocity do not seem to be too different from those in Luhar et al. (2017).

- Page 25, lines 11 – 12: The comment "This is most likely reflecting the ongoing industrialization process of countries in the southern hemisphere and the commitment and implementation of air quality regulations of industrialized nations in the northern hemisphere" is quite speculative and may be omitted.

- Eq. (13) cf. Eq. (14): $g_S TOorG_s to$ – use consistency with notation.

- The first half of the abstract, the text before "In this paper...," is introductory material and can be deleted.

- In the abstract (lines 15-16), it is better to say "...leading to an increase in surface ozone of up to 100% in some regions."

- Page 22, line 7: "At about 4 of 6 sites." About? Not sure?