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 respond to 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. We elaborate on the source of the *"largest impact on global scales"* in Section 3.2.2 (P18 L4–11). But we have to admit that it might not be clear to which resistance term the changes in the prescribed dry deposition velocities apply.
  - Is it the surface resistance (R_c) value? Or the other two resistances (R_a and R_b)? What are the typical values? It would be indeed interesting to look at the resistance terms separately. However, they are not available in our output files. Technically, it would be possible to force the model output but such would involve redoing the experiments and run for at least a couple of (model) weeks. In our formulation, the surface resistance R_c includes both stomatal and non-stomatal conductance. In case, of non-vegetated surfaces, such as *desert*, *ocean*, and *snow/ice*, the non-stomatal conductance, $G_{\mathrm{ns}}^{\mathrm{O_3}}$, is dominant. For water, $R_b = 10\,\mathrm{s\,m^{-1}}$ in most cases which is 2 orders of magnitude smaller than R_c. Thus, $R_{\mathrm{gs}}^{\mathrm{O_3}} \approx R_c^{\mathrm{O_3}} \propto (v_{\mathrm{DD}}^{\mathrm{O_3}})^{-1}$ (for R_c values see Table S2 in the manuscript Supplement S.3).
  - What value of R_c for water has been used and how it compares with the value used in the Wesely scheme? We have given the prescribed dry deposition velocities in Section 3.2.2:
    - For the *Wesely scheme* $v_{\mathrm{water}}^{\mathrm{O_3}} = 0.07\,\mathrm{cm\,s^{-1}}$ and
    - for the *EMEP scheme* $v_{\mathrm{water}}^{\mathrm{O_3}} = 0.05\,\mathrm{cm\,s^{-1}}$.

    Surface resistances for water are thus $R_c \approx 1429\,\mathrm{s\,m^{-1}}$ (*Wesely scheme*) and $R_c = 2000\,\mathrm{s\,m^{-1}}$ (*EMEP scheme*), respectively.

- P22 Table 3:
  - Why the deposition values for ocean + ice + land do to add up to the total values reported, for all simulations? Yes, that is, for we exclusively selected gridboxes associated to more then 98% to the three surface types (ocean, ice, and land), while *total* comprises all gridboxes. We admit that this is not clear. We shall repeat the computation of the values using the proper weighting and adapt the numbers (Table 1). We may also add that *ice* in this analysis only refers to regions at high latitudes that are permanently covered by ice and snow and hence does not take sea ice and mountain glaciers into account. In the Oslo CTM3, as written elsewhere in the manuscript, we compute dry deposition on ice and snow based on meteorological data. Therefore, the given

Table 1: Total ozone dry deposition for the respective model experiment in Tg a^{-1}. The global ozone dry deposition has been weighted by ocean, ice and, land fraction in each gridbox, respectively. *Ice* herein refers to regions at high latitudes that are permanently covered by ice and snow.

	Ocean			Ice			Land		
Experiment	NH	SH	Global	NH	SH	Global	NH	SH	Global
	(Tg a^{-1})			(Tg a^{-1})			(Tg a^{-1})		
Wesely_type	159.7	147.8	307.5	3.6	6.3	9.9	446.6	193.7	640.3
EMEP_full	107.6	105.2	212.8	2.5	5.5	8.0	296.6	135.0	431.6
EMEP_offLight	110.0	105.8	215.8	2.5	5.4	7.9	337.1	156.0	493.0
EMEP_offPhen	108.0	105.3	213.2	2.5	5.5	8.0	301.6	139.1	440.7
EMEP_SWVL4	109.2	107.0	216.2	2.6	5.5	8.1	303.6	138.2	441.8
EMEP_ppgs	107.6	105.2	212.7	2.5	5.5	8.0	299.5	135.3	434.9
EMEP_ppgssh	108.3	105.4	213.6	2.5	5.5	8.0	306.8	142.8	449.6
EMEP_ppgssh_ice	107.4	105.3	212.7	1.2	1.6	2.8	303.0	144.1	447.2
EMEP_ppgs_2005	106.9	103.5	210.4	2.6	5.4	8.0	297.9	131.8	429.7

values in Table 3 comprise ozone dry deposition on sea ice in the case of *ocean* and on snow covered land during winter in case of *land*.

- 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? Güsten et al. (1996) conducted measurements of ozone concentrations and fluxes onto the Sahara desert and deduced dry deposition velocities. We have not found any recent paper conducting field experiments in desert regions. We include a more thorough discussion of our results with respect to the observed fluxes by Güsten et al. (1996) and the references therein the revision of our manuscript.
  - 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. Thank you for the advise. We will look into this.
- It will also be useful to report the global ozone burden from the various simulations. Since Table 3 is already at maximum width with respect to the the page width, we shall show the global tropospheric ozone burden in a separate table (see the following Table 2). If we compare our results with Stevenson et al. (2006) (344 ± 39 Tg) and the number given in IPPC AR5 (2013)

(337 ± 23 Tg), we find that the ozone burden in the Oslo CTM3 is higher then the model average from the start (*Wesely scheme*). The implementation of the *EMEP scheme* increases the tropospheric burden by roughly 8 % (compare *Wesely type* and EMEP_full).

Table 2: Annual mean tropospheric ozone burden for all experiments and 1σ standard deviation.

Experiment	Trop. O_3 (Tg)		
Wesely type	361	±	21
EMEP_full	392	±	28
EMEP_offLight	388	±	26
EMEP_offPhen	392	±	27
EMEP_SWVL4	402	±	31
EMEP_ppgs	392	±	28
EMEP_ppgssh	391	±	27
EMEP_ppgssh_ice	403	±	31
EMEP_ppgs_2005	386	±	26

- 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). The reviewer is indeed correct that this equation is wrong. In fact, we originally used Eq. (2) with L in the denominator for the second term. The sign error was likely the reason why the *Monteith method* was chosen. Certainly, an update shall be considered in the future, but it is not feasible to redo all simulations now. In the revised version of the manuscript, we change the text from *"In Simpson et al. (2003,2012) it is described as [...] fall back to the [...]"* to *"For technical reasons, we have used the [...]"*.

- 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. The Oslo CTM3 is properly introduced in Section 2. Hence, we move the sentence in L30-32 about *polar boundary layer ozone depletion* to Section 2: *"Although the ozone depleting events in the polar boundary layer (Section 1) are important to understand surface ozone abundance in Arctic regions in spring-time, no parameterization of these processes is implemented in the Oslo CTM3 as of now."*
  - 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. Given that the influence of VSLS on tropospheric ozone is indirect (through depletion of ozone in the upper troposphere – lower stratosphere and subsequent STE), this reference (L32–33) rather belongs to the discussion in Section 4 and will be moved: *"In particular, the STE depends on the stratospheric ozone abundance which is, e.g. affected by very short-lived ozone depleting substances (VSLS) (Warwick et al., 2006; Ziska et al., 2013; Hossaini et al., 2016; Falk et al., 2017) and is not taken into account in the Oslo CTM3."*

- 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 $722.8 \pm 87.3\,\mathrm{TgO_3yr^{-1}}$, which includes an oceanic component of $98.4 \pm 30.0\,\mathrm{TgO_3yr^{-1}}$. 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."*

- P11 Section 2.2: Since the present paper is about ozone dry deposition, this section seems like a distraction and hence should be omitted. The referee is right in his/her assessment. We therefore omit this section in the revised version of our manuscript.

- P14 L15-18: Anthropogenic, biomass burning, and biogenic emissions are included in the model. How are other emissions such as soil NO_x, wetland methane, and oceanic methane and CO specified? Emissions from soil and wetlands are computed by MEGAN. Resultant NO_x emissions are upscaled to match Global Emis-

sions InitiAtive (GEIA) inventory. For oceanic emissions of CO, we use predefined global fields (POET, available through ACCENT/GEIA, `http://accent.aero.jussieu.fr/database_table_inventories.php`). CH_4 is taken from surface data from the EU project Hydrogen, Methane and Nitrous oxide: Trend variability, budgets and interactions with the biosphere (HYMN; EU GOCE 037048) for the year 2003 and scaled to oceanic amounts of CH_4 from NASA (`https://www.esrl.noaa.gov/gmd/ccgg/trends_ch4/`) given for the years 2000–2004. We shall include this information in the revised manuscript.

- P15 L4: 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? In the introduction to Section 3.1, we wrote *"For all model integrations, the meteorological reference year is 2005. This choice only affects the comparison with data and multi-model studies that either perform analysis on decadal averages or differing years."*. We will clarify the sentence with respect to probable consequences.

- Section 3.2.1:
  - 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. We agree that ozone dry deposition to oceans is of high importance for the southern hemisphere, last but not least in the zonal band $(50-70)°$S. In fact, we have mainly discussed out results with respect to seasonal cycles of dry deposition velocities onto ocean found in Hardacre et al. (2015) (Fig. 7; P21 L1–10: *"Similarly, the dry deposition velocities over water differ. [...]"*). We will include a discussion based on Fig. 5 in the revision of the manuscript.
  - 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). We regard this remark as a follow-up of the question raised regarding P15 L4. We will elaborate on the discussion regarding the implications based on our model results (EMEP_ppgs vs EMEP_ppgs_2005) in the revision of the manuscript. Though, the major consequence of this is that, for the majority of our model experiments, one can neither directly compare to observations for the years 2005 and 2014 nor to other the model results. Emission inventories may always be lacking in certain chemical species and the non-linearities in ozone formation and destruction make ozone concentrations sensitive to both emissions of precursors and meteorological conditions. Surface ozone observations, in fact, show a strong variability in ozone dry deposition and ozone concentrations at the sites. But studying these in detail may be well beyond the scope of this manuscript.

- P24 L3–4: *"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. We have indeed not specified our statement in the mentioned sentence, while we had done so elsewhere in the manuscript (P15 L12). We complete our statement in the revised version of the manuscript: *"[...] ozone dry deposition decreases by up to* 100 % *over all major desert areas [...]. At the same time, it increases over tropical forest.*

- P24 L15: *"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 (R_c) term? For the ocean, it is likely to be R_c. For deserts, maybe R_b? Is it possible to quantify these differences in the resistance terms? We have already answered the question with respect to ocean (see first bullet point). In summary, since R_b is quite small in most of the cases, the dominant factor for the ozone dry deposition onto ocean is the surface resistance R_c which is tabulated in Table S2. Regarding ozone dry deposition onto deserts, we use Eqs. (7–8) to deduce

$$R_b^i = \frac{2}{\kappa u_*} \cdot \left(\frac{D_{\mathrm{H_2O}}}{D_i} \cdot \frac{\mathrm{Sc_{H_2O}}}{\mathrm{Pr}} \right)^{\frac{2}{3}}, \tag{1}$$

with $\mathrm{Pr} = 0.72$, $\kappa = 0.4$, $\mathrm{Sc_{H_2O}} = 0.6$, $D_{\mathrm{H_2O}}/D_i = 1.6$. We estimate u_* from Eq. (16.67) in Seinfeld and Pandis (2006)

$$u_*(h) = \frac{\kappa \cdot \overline{u_x}(h)}{\ln(h/z_0)}, \tag{2}$$

with $h = 8\,\mathrm{m}$, $z_0^{\mathrm{desert}} \approx 10^{-3}\,\mathrm{m}$, and wind speeds not exceeding a gentle breeze ($1.8\,\mathrm{km\,h^{-1}} \leq \overline{u_x}(h) \leq 28\,\mathrm{km\,h^{-1}}$), we find $272\,\mathrm{s\,m^{-1}} \geq R_b \geq 17\,\mathrm{s\,m^{-1}}$. This is $1-2$ orders of magnitude smaller than $R_c = 2000\,\mathrm{s\,m^{-1}}$ and thus not negligible for low wind speeds. In summary, R_c is dominant in our formulation of dry deposition of ozone to deserts (unless we have calm wind conditions).

- P24 L24: *"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). We acknowledge Luhar et al. (2018) an update the sentence: *"[...] 2-layer gas exchange with ocean waters (Luhar et al., 2017, 2018)."*

- P25 L11–12: 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. We follow the kind advise of the referee and remove the sentence in the revised version of the manuscript.

- Eq. (13) cf. Eq. (14): g_{STO} or G_{sto} – use consistency with notation. Thanks for pointing this out. We will change this in the revised version of the manuscript.
- The first half of the abstract, the text before *"In this paper...,"* is introductory material and can be deleted. This is indeed the case and we will remove it in the revised version.
- Abstract L15–16: it is better to say "...leading to an increase in surface ozone of up to 100% in some regions." We follow the advice of the referee and change the sentence accordingly.
- P22 L7: *"At about 4 of 6 sites."* About? Not sure? Thanks for pointing out the misplaced *"about"* in this sentence. We are certain regarding that number.