Authors' response

To gmd-2019-21 referee#2 David Simpson (10 Apr 2019): We apologize for the confusion the misuse of the terms *EMEP* and *EMEP scheme* may have caused. We evaluated the major concerns raised in the review and found them severe enough that we have to revise our model and repeat the model experiments to address all concerns. Nevertheless, we will respond to all of the questions and concerns in more detail in the following.

1 Major points

- Mosaic formulation:
  - CTM3 claims to implement a mosaic approach, but instead of calculating deposition rates over each land-cover, and then aggregating using Eqn.3, CTM3 seems to perform the following steps:

$$R_a = \frac{u_z}{u_*^2} \tag{1}$$

$$\overline{Gs_g^i(z)} = \Sigma_{k=1}^N f_k \times Gs_{g,k}^i(z) \tag{2}$$

$$\overline{Gns_g^i(z)} = \Sigma_{k=1}^N f_k \times Gns_{g,k}^i(z) \tag{3}$$

As far as I can tell from the text, R_a is calculated once, with the same value of u_* for all land-covers. The CTM3 R_b calculation seems to also use the same u_* over different land-covers, except over sea where a more sophisticated scheme is used. Equations 2–3 above are equivalent to Eqns (22) and (26) from the manuscript. Now I am puzzled however as to how all this is put together. Do they use:

$$G_c = \text{LAI} \cdot \overline{Gs_g^i(z)} + \overline{Gns_g^i(z)} \tag{4}$$

Thank you very much for your detailed account of the *mosaic approach*. It is correct that we use Eq. (2) and Eq. (3). As pointed out, we have not properly indicated the weighted mean in Eq. (22) and Eq. (29) in the manuscript. Additionally, there is also a typo in Eq. (13) and Eq. (22) (G_{sto} should have been g_{sto}). Eq. (22) should have read as follows:

$$\overline{g_{\text{sto}}^{\text{m}}} = \sum_{n=0}^N f_{\text{L},n} \cdot g_{\text{sto},n}^{\text{m}} \tag{5}$$

All is put together in Eq. (13):

$$G_c = \text{LAI} \cdot \overline{g_{\text{sto}}^{\text{m}}} + \overline{G_{\text{ns}}} \tag{6}$$

We acknowledge that Eq. (13) should not procede the other equations and that this might have caused additional confusion.

    – In any case, I think this approach has serious problems. Why average first for G_s and then for G_{ns} , when it is the fluxes (F_k , or $V^i_{g,k}(z_{ref}) \times \chi^i_{avg}(z_{ref})$) which need to be averaged? I also do not understand why they would use the same u_* and R_a for all land-covers. I think the authors need to make a case for their approach, or change it. We have discussed the concerns raised within this comment and found that we have indeed made a mistake in our implementation of the *mosaic approach* which forces us to revise our model and repeat the model experiments.

- Calculation of R_a:

    – R_a in CTM3 appears to be calculated just once, and from a height of 8 m. This means that there is no consistency between the R_a term and the underlying surface, which is clearly wrong. The similarity equation for R_a given in their Eqn (2) is very standard and has been used for decades (Garratt 1992). As pointed out by Ref 1, the equation as given has errors. Eq. (2) in our manuscript is indeed erroneous. The correct equation is (e.g., Erisman, 1994):

$$R_a = \frac{1}{\kappa u_*} \left[\ln\left(\frac{z-d}{z_0}\right) - \Psi_m\left(\frac{z-d}{L}\right) + \Psi_m\left(\frac{z_0}{L}\right) \right]. \qquad (7)$$

    – The correct equation will not give negative values unless presented with wrong inputs, and I suspect that that is what has happened. It is actually difficult to tell what was tested from the manuscript though, since they state simply that d is 'typically 0.7 m'. Did they use d properly, consistent with the underlying land-cover and its z_0? Did they assume that their 8 m meteorology was at a physical height of 8 m, or at $d + 8$ m. If the latter, which d? At that point, it is indeed neither clear from the manuscript nor from our internal documentation. The assumption that Eq. (2) (in the manuscript) would become negative, was most likly based on a wrong form of the integrated stability function Ψ_m.

Given $z = z_{ref} = 8$ m and $d = 0.7 \cdot 1$ m, it is right that neither the correct Eq. (7) nor the erroneous equation will result in negative R_a. The statement "d is 'typically 0.7 m'" is a typo. We correct: "[...] typically $d = 0.7 \cdot h(N, \text{lat})$ m *for vegetation other than forests [...]*".

Correction note: The average height of the lowermost model level is actually 20 m (10 m for mid-level).

    – Lines 19–30 of this section explain the Monteith alternative, but in a rather confusing way. For example, when is z_0 ever zero, as stated on line 23, or why does $\partial_z R_a \to R_a$ for finite z? (I know what they intend to say, but it isn't at all clear.) The referee is right. The text and derivation of the Monteith relation is confusing and also erroneous. If we do not implement Eq. (7), we will elaborate on this in a revised version of the manuscript: "

$$\Phi_{Q_{sensible}} = \rho_{air}\, c_P \cdot \frac{\Delta T}{R_{a,H}}. \qquad (8)$$

Herein, ρ_{air} is the air density, c_P the specific heat at constant pressure P, $R_{a,H}$ is the diffusion resistance to sensible heat between surface and the reference height z, and ΔT is the difference between the surface temperature T_0 and the temperature at a reference height T_z. From the eddy diffusion theorem, a similar formulation can be derived

$$\Phi_{Q_{sensible}} = \rho_{air} \, c_P \cdot \frac{\partial T}{\partial u} \cdot u_*^2. \tag{9}$$

For finite z, $\partial T \rightarrow \Delta T$. "

– In any case, here the authors end up with a stability-independent equation for R_a, without mentioning or discussing that fact. In the revison of the model, we are going to implement the stability dependent R_a following Eq. (7).

– This very shallow layer is also very problematic for deposition calculations in general, since the model cell seems to be run here with horizontal dimensions of $2.25 \times 2.25°$, or about $250 \times 250\,\text{km}$ near the equator, but a vertical mid-level (CTM3's z_{ref}) of just $8\,\text{m}$. Now, profiles of wind and depositing gases are very sensitive to the underlying surface, and should be very different for forests or lakes for example. Any wind-speed or friction velocity calculated from a model of such large horizontal resolution will necessarily give values at $8\,\text{m}$ which reflect the whole grid. Deposition rates for a specific land-cover will vary enormously depending on what else is in the grid-square. (Although not strictly comparable, we showed in Schwede et al. (2018) that differences between the grid-average and forest specific deposition rates of N-compounds could be as much as a factor of two and up to more than a factor of five in extreme cases. These differences were largely dependent on how much forest occupied each grid cell.) Thank you for pointing this out. In S2012 we find, R_a evaluated "at around $45\,\text{m}$" which is similar to the center of our second lowest model level. We can easily correct for this in a revised model version.

• Why so much focus on sea areas?: The text seems rather unbalanced with regard to the different land-covers. Sect. 2 uses $1/2$ page on various z_0 corrections for oceans, but say nothing about the ecosystem where ozone is expected to deposit at high rates: forests, crops, and other terrestrial ecosystems. The supplementary has three Figs related to this oceanic deposition. Why? The referee is right that ozone is predominately deposited to terrestrial ecosystems, which cover about $2/9$ of the Earth's surface, while $2/3$ is covered by oceans. Nevertheless, due to the ocean's vastness, relatively small changes in modeled dry deposition easily accumulate and influence the atmospheric ozone concentration. Luhar et al. (2017, 2018) show that the current models may overestimate the oceanic ozone sink by a factor of three – rendering the ocean a even smaller sink for ozone.

This said, Section 2 as a whole consists of about 9 pages, of which $1/2$ page is dedicated to oceans (R_b computation) and 7 pages are dedicated to vegetation especially stomatal and non-stomatal conductance (R_c computation). Hence, we do not see a significant unbalance in favor of oceans herein.

Regarding the three figures with respect to ocean in the supplementary material: The reason for these is that we found some "legacy code" in the model (linear fit to dynamic viscosity of air $\mu(T)$) after we had finished the implementation of the new dry deposition scheme, run the experiments, and done the analysis on these. We had to verify that this had no significant influence on the results.

- Use of the term 'EMEP scheme'?: Sect. 3 discusses the comparisons of V_g in terms of 'EMEP scheme' versus 'Wesely scheme', and sensitivity tests are named e.g. 'EMEP_offlight'. As noted above the scheme implemented in CTM3 is very different to that implemented in the EMEP model, so this is very misleading. Please rename your scheme to something else. I am worried that readers might get the impression that it is the EMEP scheme which is being tested here, but it certainly is not. [...] We apologize for the misleading naming of the new dry deposition scheme in the Oslo CTM3 which is (partly) based on the formulations in the publication the referee refers to as *S2012*. By no means have we meant to offend any of the original authors of the EMEP/MSC-W model nor intended to misguide the readers. We will rename the revised scheme appropriately ($\rightarrow mOSaic$ *scheme*).

- Reproduction of material from S2012:
  - As far as I can see, Table S1, S2 and S3 are taken directly from S2012, with no change to parameters. It is not usual to copy tables from the work of other authors in this way. Just refer to S2012 (and give Table number as help). We will follow this advice and refer to the tables in S2012 accordingly.
  - Many of the equations are from S2012, and many not. I would like the authors to make this very explicit, so that readers are not confused as to what comes from EMEP, and what has changed for CTM3. We are going to elaborate on this matter in a revised version and refere to our equations' sources more properly.

2 Minor points

- P1 L22. Isn't H_2O the most important greenhouse gas? (Say anthropogenic GHG perhaps?) Thank you for pointing this out. We follow the advice and write: *"[...] ozone is a potent anthropogenic greenhouse gas [...]"*

- P2 L3. The Wilson ref only concerns Europe, and its focus on the 95th percentile can hide trends found at higher percentiles (e.g. Simpson et al. 2014). A better ref would be Fleming et al. (2018) or Mills et al. (2018a). By the way, the most recent calculation on food security (using flux approaches) is now Mills et al. (2018b).

Thank you for pointing out these publications. We take them into consideration, when revising the manuscript.

- P2 L11. What does in situ mean here? Ozone production can take place over days of transport. *In situ* typically means *on site, locally.* In this context, we use it exactly in this meaning. We elaborate on this in a revised version of the paper: *Elevated ozone levels at a site originate rather from the local production of ozone from its precursors, which are transported, than from advection of ozone itself. Although long-range ozone transport occurs and might be most important in regions that else lack precursors. Tropospheric ozone is produced in complex photochemical cycles involving precursor gases [...]*

- P2 L25–35. This text about halogens is not really relevant to a dry deposition paper. Reactions with bromine can be important sinks, but are not usually counted as deposition. The removal of ozone from the Arctic boundary layer in the presence of halogens is indeed not directly related to ozone dry deposition, but one of the discussed mechanisms to trigger the bromine explosions involves, e.g., an "initialization" by ozone dry deposition. We see that this is of no relevance to ozone dry deposition itself, but to the observed ozone abundances in the Arctics. We remove this text in the revised version of the manuscript.

- P3 L2–3. Why specify mid-June maximum for ozone. Monks (2000) might take issue with that, as would for example Sinha et al. (2015). The referee has got a point here. Of cause the maximum of ozone in the annual cycle depends on the location. We remove this specification.

- P3 L4–5. There are plenty of ozone measurements made outside Europe. The authors appear to be unaware of the massive ozone collections made under the TOAR project (see e.g. Flemming, Mills refs below), or the high quality data available from GAW (inha 2015). Thank you for reminding us of the data collected under the TOAR project. We knew about this data set but have to admit that we have not made much use of it, yet. We have, however, not stated that there are, in general, no sites outside of Europe, but that long-term observations (started before the 1950s) are only found in Europe. Maybe the term "long-term" is not clear enough. We add: *"[...] a limited number of long-term ozone observations (started before the 1950s)[...]"*

- P3 L16. One also has dry deposition to water, as this paper makes clear later on. That is indeed true, but we didn't want to name all possible surfaces onto which dry deposition takes place. We, hence, shall write: *"Removal of any substance from the atmosphere which is not involving rain, e.g., through gravitational settling or by uptake by plants and soil, is referred to as dry deposition.*

- P3 L20. One usually refers to dry deposition as something between a near- surface height (e.g. $z = 1m$, $10m$, or $50m$) and the surface, not from z_0. In fact, at $z = z_0$ one has $u_z = 0$, and hence the author's R_a just below should be zero. Thank you for pointing out this inaccuracy. We, of cause, meant "near-surface" height or lowermost model level (which is indeed not at $z_0 = 0$). We change the

text accordingly: *"[...] dry deposition is a product between near-surface ozone concentration $[O_3](z_0)$ (e.g. the lowermost model level) [...] "*

- P4 L20. I would remove the term textbook knowledge, since there are many different approaches to nearly all these equations. It is thus good that the equations as used in CTM3 are spelled out explicitly. We follow the advice and remove the sentence in which the term occurs and write: *"[...] we will give a detailed account of the new dry deposition scheme and the equations that we use."*

- P5 L5. I would add Emberson et al. (2000a) and Tuovinen et al. (2004) to the list of EMEP refs here, since this was the first publication of the methods that have more or less been used until today. We have added the references regarding the EMEP MSC-W model. *"[...] we follow the European Monitoring and Evaluation Programme (EMEP) MSC-W model (Emberson et al., 2000; Simpson et al., 2003; Tuovinen et al., 2004; Simpson et al., 2012), [...]"*

- P7, notation. In S2012 and EMEP generally, we use upper-case G and R to refer to canopy-scale (bulk) variables, and lower-case for leaf-scale. Thus, in EMEP we would have $G_{sto} = LAI g_{sto}$. Here the authors seem to mix upper and lower case between their equations (13) and (14). We have indeed mixed up the cases here.

- P7 Eqn (13). Is LAI one-sided, 2-sided, projected .... define. LAI is one-sided taken from ISLSCP2 FASIR. We add this to the sentence: *"[...] LAI is the one-sided leaf area index taken from ISLSCP2 FASIR [...]"*

- P8 L1. S2012 do not suggest using depths lower than 1 m. We use SMI3 which is from 28-100 cm. Thank you for the clarification. We might have mixed this up. Since we have to repeat our simulations, we will choose SWLV3 from the start.

- P8 L2. Why did you choose to use the surface (0–7 cm) soil moisture? The reason for the initial choice of 0–7 cm came from the project this work is related to. In our project, we are interested in ozone in boreal regions where the soil column is generally more shallow.

- P8 L18. This is wrong. Nothing in the EMEP model is used to 'mimic the time lag..'. We use the light function to modify stomatal conductance, as with the other f factors. We change this inadequate formulation: *"[...] this integrated photon flux is used to modify the stomatal conductance in response to light."*

- P9 L20, and Table 1. The consequence of Table 1 is that vegetation at $0.5°$ N will start growing at day 90, whereas those at $0.5°$ S will start on day 272. (By the way, in EMEP now we use monthly factors from the LPJ-GUESS model to derive phenology for non-European areas, because of such difficulties with tabulations.) Thank you for the remark. We see this problem. Using an actual land model would solve this, but at that point it is not feasible to integrate such in the Oslo CTM3.

- P9 L18. What do you mean by "surface or $2\,\mathrm{m}$"? Surface might refer to skin or leaf temperature? This may have been indeed unclear. It is the $2\,\mathrm{m}$ temperature. We change the text accordingly: "[...] θ_0 *is the* $2\,\mathrm{m}$ *temperature in* $^\circ\mathrm{C}$."

- P9 Eqn (26). Say 1st and 2nd, not I. and II. We follow the advise regarding Eq. (26) and also change Table 1 accordingly.

- P9 Eqn (27). This equation is a modification of Erisman's original (1994) formulation, so explain that. We follow the advise and write: "*The in-canopy resistance* R_{inc} *(Erisman et al., 1994) is then modified with respect to each (vegetated) land type* N *[...]*" **ToDo – To Amund: Were does this modification come from?**

- P10 L12. Be explicit that this statement refers to S2012. The current EMEP model uses different heights for e.g. tropical vegetation. We follow this advice and write: "*The vegetation height* $h(N, lat)$ *as described by Simpson et al. (2012) [...]*"

- P11 Sect 2.2. I also found this aerosol section confusing. Eqn (30) is from S2012, and so is the factor 0.008.SAI/10 used in Eqn (33), but here new a_1 coefficients are defined. Did the 'aerosol microphysic model' referred to also mix equations in this way? Is there any publication as to the reliability of this method? The a_1 coefficients were calculated from deposition velocities taken from the Oslo CTM2 which has been documented **ToDo: Citation of CTM2?**. We have actually not used this new scheme for aerosols in the Oslo CTM3, yet. Hence, we will remove this section from the paper.

- P12 Fig.2. I didn't understand what is being done here. The Figure suggests that the EMEP scheme has one category for 'Forests, Med. scrub', whereas S2012 lists 4 types of forest, as well as Mediterranean scrub as a separate ecosystem. This figure also suggests that EMEP has savanna, which it doesn't, but do have many other categories (Table 3 of S2012 lists 16 main categories. The current EMEP model has 32.) The caption may become clearer after we changed the name of our scheme, so that it can no longer be confused with the actual EMEP model. The purpose of the mapping is to project the more detailed categories for stomatal conductance to the 10 ground-surface resistance categories (Table S19 in S2012 supplement).

- P12 L13. Again, the current EMEP model is not eurocentric, and uses global phenology calculations. We follow the advice and drop the term "eurocentric" the text: "*Since the parameterization of SGS and EGS in Simpson et al. (2012) is not applicable [...]*"

- P13 Sect 2.3.2. The initial lines (14-16) are hard to understand and only by reading further do I see what they mean by 'de-accumulated'. If working with IFS PPFD is so hard, why didn't the authors just calculate hourly (or minute-by- minute) PPFD using cloud-cover and zenith angles? We have not used that ansatz, because we do not have the radiation fields in the relevant wavelengths available from OpenIFS output.