Review of Falk & Haslerud, Update and evaluation of the ozone dry deposition in the Oslo CTM3 v1.0

David Simpson, EMEP MSC-W, April 2019

This paper summarises an update in the dry deposition framework of the Oslo CTM3 model. Many of the equations and ideas are taken from the EMEP model of Simpson et al. (2012) (which I will refer to as S2012 below), but the equations are applied in ways that seem to differ substantially from the S2012 approach. Although differences are expected, I have trouble following the logic behind the CTM3 formulation, and in some cases I would say it is incorrect.

I apologise for a somewhat harsh tone below, but I am afraid to say that I found too many difficulties with both the concepts and the clarity of this paper. I cannot recommend the paper for publication without major revision to both the paper and probably the underlying model construction.

Major points

A) Mosaic formulation

In the EMEP model, the deposition flux ($F_{g,k}^i$) of a gas i to the ground surface over ecosystem k is modelled using the so-called deposition velocity, $V_{g,k}^i(z)$, such that:

$$F_{g,k}^i = -V_{g,k}^i(z) \times \chi_k^i(z) \tag{1}$$

This equation is assumed to be true throughout the so-called constant flux layer. In S2012 we assume that the concentration and deposition velocity calculated at the centre of the lowest grid cell (~ 45 m in S2012, a height we refer to below as the reference height z_{ref}) is within this layer, and thus we can set all $\chi_k^i(z_{ref})$ equal to one average value, $\chi_{avg}^i(z_{ref})$. The deposition velocities over each land-cover are calculated using standard big-leaf approaches as:

$$V_{g,k}^i(z) = \frac{1}{R_{a,k}(z) + R_{b,k}^i + R_{c,k}^i} \tag{2}$$

To calculate grid-average deposition fluxes, EMEP implements a so-called mosaic approach, whereby the the grid-average deposition rate is given by:

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

where the overline symbolises the grid-square average, f_k is the fraction of land-cover type k in the grid-square, and $V_{g,k}^i$ is the deposition velocity for this land-cover type. An important point about the S2012 approach is that it calculates all terms (including u_*, R_a, etc.) consistently for each land-cover, and then averages the fluxes.

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:

$$R_a = \frac{u_z}{u_*^2} \tag{4}$$

$$\overline{Gs_g^i(z)} = \sum\nolimits_{k=1}^N f_k \times Gs_{g,k}^i(z) \tag{5}$$

$$\overline{Gns_g^i(z)} = \sum\nolimits_{k=1}^N f_k \times Gns_{g,k}^i(z) \tag{6}$$

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 5-6 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:

$$Gc = \text{LAI } \overline{Gs_g^i(z)} + \overline{Gns_g^i(z)} \tag{7}$$

as suggested by their Eqn (13) (though I added overlines here), followed by $R_c = 1/G_c$ and then the manuscript's Eqn (1) for deposition velocity? (In this case, which LAI is used?)

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 $V_{g,k}^i(z_{ref}) \times \chi_{avg}^i(z_{ref})$) 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.

B) 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. 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?

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.) In any case, here the authors end up with a stability-independent equation for R_a, without mentioning or discussing that fact.

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 $250\,\text{km} \times 250\,\text{km}$ near the equator, but a vertical mid-level (CTM3's z_{ref}) of just 8 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 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.)

C) 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?

D) Use of term 'EMEP scheme'?

Sect. 3 discusses the comparisons of Vg 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. For those interested, the EMEP scheme itself, and its stomatal conductance formulation, have been discussed extensively in a series of papers, e.g. Emberson et al. (2000a,b, 2001), Klingberg et al. (2008), Simpson et al. (2001, 2003), Tuovinen et al. (2001, 2004, 2009).

E) 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).

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.

F) Other

1. p1, line 22. Isn't H_2O the most important greenhouse gas? (Say anthropogenic GHG perhaps?)

2. p2, line 3. 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).

3. p2, line 11. What does *in situ* mean here? Ozone production can take place over days of transport.

4. p2, lines 25-35. 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.

5. p3, line 2-3. Why specify mid-June maximum for ozone. Monks (2000) might take issue with that, as would for example Sinha et al. (2015).

6. p3, line 4-5. 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).

7. p3, line 16. One also has dry deposition to water, as this paper makes clear later on.

8. p3, line 20. 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.

9. p4, line 20. 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.

10. p5, line 5. 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.

11. 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 $G_{sto} = \text{LAI } g_{sto}$. Here the authors seem to mix upper and lower case between their equations (13) and (14).

12. p7, Eqn (13). Is LAI one-sided, 2-sided, projected .... define.

13. p8, line 1. S2012 do not suggest using depths lower than 1m. We use SMI3 which is from 28-100 cm.

14. p8, line 2 - why did you choose to use the surface (0-7cm) soil moisture?

15. p8, line 18. 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.

16. p9, line 20, 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.)

17. p9, line 18. what do you mean by "surface or 2m"? Surface might refer to skin or leaf temperature?

18. p9, Eqn (26). Say 1st and 2nd, not I. and II.

19. p9, Eqn (27). This equation is a modification of Erisman's original (1994) formulation, so explain that.

20. p10, line 12. Be explicit that this statement refers to S2012. The current EMEP model uses different heights for e.g. tropical vegetation.

21. 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?

22. 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.)

23. p12, line 13. Again, the current EMEP model is not eurocentric, and uses global phenology calculations.

24. 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?

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