# Interactive comment on “Impact of future land cover changes on $HNO_3$ and $O_3$ surface dry deposition” by Verbeke et al.

## General Impression

The manuscript by Verbeke et al assesses the impact on land cover change on the deposition velocity and, by association, deposition and air concentration of $HNO_3$ and $O_3$ and tries to compare the impact with the isolated impact of climate change. The subject of interest and worth of publication, but the current manuscript lacks clarification of what was actually done, a discussion of the uncertainties and limitations of the approach as well as a discussion of the results in the context of other studies. It therefore requires significant modifications before it can be accepted for publication in ACP. I am very disappointed that most of my technical comments provided at the access review stage for ACPD have not been taken on board, including some clear spelling mistakes which I now have to repeat here.

## Major comments.

1. As with any Chemistry Transport Models and even more so, Earth System Models, there are hidden interactions within the model that complicate the interpretation of the results and much depends on the exact implementation. Looking at the impact of one change (LUC or climate) in isolation is difficult both technically (because of the links within the modelling system) as well as philosophically (because in reality changes do not occur in isolation). Therefore, for the reader to be able to understand the implications of the work, it is essential that the authors provide as much detail as possible on what which part were kept constant between runs and what was changed. This should be added to Section 2. At present much of the detail is obscure or is clarified very late in the manuscript:
    a. The authors should provide some more detail on the Wesely parameterisation and clarify further to what extent their implementation matches that of Wesely. For example, what parameterisation is used for Rb, the values of which differ between parameterisations in the literature, especially for aerodynamically rough surfaces (i.e. forests).
    b. For example, no information is given on the calculation of the stomatal conductance and the authors should confirm in the manuscript that this follows Wesely as it is often generated independently in CTMs. If it does follow Wesely (as implied only by the very last paragraph of the discussion section), it is not increased by $CO_2$-related increases in gross primary productivity or lowered by the ability of plants to reduce water loss whilst maintaining photosynthesis under higher $CO_2$ concentrations. The feedback of changes in $O_3$ itself on plant growth and stomatal conductance (Sitch et al., 2007) is then also not captured.
    c. Similarly, it is unclear whether the model calculates a single u* and heat flux for each grid cell or whether this is calculated for each land cover class individually. If the former, the implications need to be discussed. Landcover change changes the roughness and heat fluxes at the landscape scale, which in reality feeds back on the meteorology. However, the authors seem to drive the LUC scenarios nudged with the same meteorology, which generates an inconsistency. Would it not be more appropriate to do free 10-year runs GCM runs similar to the climate change scenario runs, but keeping atmospheric composition and sea surface temperature and also ice cover (which is not mentioned, but is presumably changed between the GCM runs?) constant. For these runs the LUC would have the ability to feedback on climate.

d. Agricultural emissions appear not to have been changed between runs. In reality, LUC induces a change in both natural and anthropogenic emissions. Therefore, it is more meaningful to compare changes in $V_d$ than in concentration and deposition.
e. Whilst BVOC emissions appear to be kept constant between runs, presumably, the deposition of other compounds, that chemically interact with $HNO_3$ (e.g. $NH_3$) and $O_3$ (e.g. $NO_2$ and VOCs) also change with landcover and climate. Thus, the change in concentration is no longer merely affected by the deposition velocity of the compounds themselves.

2. Comparison with measurement data, limitations of the Wesely approach.
   a. The paper lacks any sort of assessment of the modelled $V_d$ with measurements (or reference to another paper that performs this assessment) and it is therefore difficult to assess how the parameterisation (and its implementation into this particular CTM) performs under current conditions and whether the predicted changes are therefore robust. The Wesely approach is now 15 years old and does not reflect the measurement evidence of the past decades, with several studies indicating, e.g., that deposition rates to wet cuticles are larger than to dry surfaces, although the process understanding is still uncertain. The Wesely approach greatly relies on static look-up table derived from measurements under current conditions which may change in the future. For example, it does not include a mechanistic understanding of the effects of leaf water chemistry on non-stomatal pathways, which may be altered by climate and composition change. None of these uncertainties and limitations of the study are discussed in the present manuscript.
   b. It seems counterintuitive why crops should provide a more efficient sink to ozone than coniferous forest, especially in winter (P18468 and first figure in Supplement), because the "LAI" of bare soil is much smaller than that of forest. Also, at present it is not clear whether the figure in the supplement reflects the global average (i.e. mixes winter and summer values) as no caption is provided.

3. I would have expected the authors to discuss findings in much more detail in relation to the published literature. How large are the expected concentration changes due to LUC compared with other effects, such as changes in emissions and chemistry? Some key references such as Hardacre et al. (2015) are not discussed.

## Minor Scientific Comments

1. I suggest the authors avoid the use of the term "deposition rate" throughout the manuscript (text & figures), because this is used ambiguously in the literature, sometimes referring to the deposition flux and sometimes to the deposition velocity. I suggest you use "deposition velocity" throughout.
2. In the abstract, please point out that the same meteorology was used for the LUC scenarios.
3. $V_d$ and $R_a$ are both height dependent, which should be indicate as $V_d(z)$ in Eq. (1). Please state throughout the height to which the values of $V_d$ refer to throughout the manuscript and in the captions to the appropriate figures.
4. Is it really the oxidation capacity that is treated by Wesely? I thought it was more generally the reactivity.
5. One of the most important factors affecting deposition rates is turbulence (P18462, L10), which also governs $R_a$ (P18464, L3) and, together with surface roughness also affects $R_b$ (L4). By

contrast, $R_c$ is probably more affected by LAI and canopy structure than by roughness length (L7).

6. P18471, L4. As mentioned above, this includes the deposition of other compounds that chemically react with $O_3$ and $HNO_3$. This also needs to mention in relation to P18471, L25.

## Technical comments (in addition to those already raised by Reviewer 1):

Please sort out the (missing) use of super- and subscripts throughout the document (incl. Supplementary Information and figures), which is very sloppy throughout and distracts from the content. Subscripts are used in the equations, but not in the text. Also, I suggest you write VdHNO3 as $V_d(HNO_3)$ or $V_{d,HNO_3}$, ideally even stating the height it refers to.

Please number figures in Supplement and add captions.

Figure 4 and 2nd figure in SM: the units of the deposition flux are still incorrect by many orders of magnitude, possibly the $s^{-1}$ should read $a^{-1}$? Please check and correct. Also, usually, these are stated as $m^{-2}$ (if averages) or maybe $ha^{-1}$ if annual totals.

Symbols VdO3 and VdHNO3 are not defined in the abstract.

P18461, L7: Better English: "… should therefore be considered …"

P18461, L10: Plays a key role in what?

P18461, L19: Add a reference for Wesely's description already here.

Table 2. In the header 'ozon' should read 'ozone'.

P18461, L26. Correct spelling of 'highly'.

P18462, L6. Should read "at the regional …"

P18462, L14. Should read "in a very simplistic way"

P18462, L15. Improve English; two occurrences of "usually" in the same sentence.

p18465, L17. Should read "in more detail"

p18466, L23. Better: "a strong increase in the cover of all forest categories"

p18466, L27. Should read "America loses"

p18467, L4. Better "dry deposition without any climate change"

p18466, L7. Specify that 2007 meteorology is used.

p18466, L23. Should read "temperature change is"

p18467, L10. Better "lower annual $V_dO_3$"

P18469, L18. Should read "by land cover change"

P18469, L27. Should read "decreasing by up to 0.2" and similar instances elsewhere in the manuscript.

P18471, L15. Better "in the east" and "in the west"

P18472, L21. Should read “assess the impact”

P18473, L13. There should be a comma after “Eurasia”

P18474, L11. Should be “show” instead of “shows”

P18474, L14. Should read “The next generation”

## References

Hardacre, C., Wild, O., and Emberson, L.: An evaluation of ozone dry deposition in global scale chemistry climate models, Atmos. Chem. Phys., 15, 6419-6436, doi:10.5194/acp-15-6419-2015, 2015.

Sitch, S., P. M. Cox, et al.: Indirect radiative forcing of climate change through ozone effects on the land-carbon sink. Nature 448(7155): 791-794, 2007.