

We thank the reviewer for helpful comments. We have no issue with the review and have gratefully made the following modifications to the paper:

*1) The modelling studies and results from this manuscript are most relevant for the hygroscopic growth of water soluble particles. This point is made clearly in the Discussion and Conclusions Section. However, I think this point should be made more clearly in the Abstract and Introduction. For example, line 17 of the abstract “this as well as other results here questions whether particle-phase diffusion can be a limiting factor in gas- particle mass transfer in the ambient atmosphere, at least for water-soluble particles”. This sentence may be clearer if they point out that they are referring to mainly water vapor-particle mass transfer. Perhaps something like the following may be clearer: “this as well as other results here question whether particle-phase diffusion can be a limiting factor in the hygroscopic growth of atmospheric particles”.*

This is a valid point, pp. 1 line 16 of the abstract and pp. 3 line 7 of the introduction have been changed to accommodate it:

" This, as well as other results here, questions whether particle-phase diffusion through water-soluble particles can limit hygroscopic growth in the ambient atmosphere"

and

" In most test cases below the diffusing semi-volatile component has the self-diffusion coefficient of water at room temperature, and the resulting diffusion timescales are most relevant to water and water-soluble particles, however, the findings regarding consistency between models are applicable to components with self-diffusion coefficients across the investigated range ( $2 \times 10^{-9}$  -  $1 \times 10^{-25}$   $\text{m}^2\text{s}^{-1}$ ). "

*2) Equation 2. For clarity, please define the variables in the equation.*

pp. 3 line 21 changed to explain the terms in Eq. 2:

" The  $e$ -folding time for the difference in concentration of the semi-volatile component at the surface ( $[sv]_{eq}$ ) and of its average concentration across the particle bulk ( $\overline{[sv]}_b$ ) was used as a metric for diffusion time by Zaveri et al. (2014). It is readily transferable to other studies, and is the chosen metric for diffusion timescale here. The ratio of the concentration difference in the surface and bulk-average of the semi-volatile component at any time ( $t$ ) to that difference at  $t = 0$  is:"

*3) Three different functions were used to describe diffusion coefficients. It would be useful to indicate why the logarithmic dependence and sigmoidal dependence were*

*chosen. For example, are these dependences consistent with theory or are they consistent with experimental results for systems like water and sucrose?*

pp. 7 line 17 was changed to give more detail on the provenance of the logarithmic dependence:

" ii)  $D_i$  with a logarithmic dependence on semi-volatile mole fraction, which has been observed for ideal systems by Vignes (1966):"

pp. 7 line 22 was changed to give more detail on the provenance of the sigmoidal dependence:

"iii)  $D_i$  with a sigmoidal dependence on  $x_{sv}$ , which was observed for the citric acid-water system by Lienhard et al. (2014):"

pp. 7 line 26 was changed to describe the advantage of using these dependencies:

"These provided a relatively steep "cliff-edge" sigmoidal dependence and therefore a substantial variation from the logarithmic dependence, enabling a test of consistency between models across a wide range of dependencies"

*4) End of section 2.3. At this location, please give more details on the system already presented by Zobrist et al. (2011), so the reader has a better idea of the type of systems the results apply to.*

pp.8 line 12 changed to be more informative of how non-ideality accounted for:

" For actual inferences one would preferably have good knowledge of the system's deviation from ideality. We therefore use the calculation of density from weight fraction presented in Zobrist et al. (2011) for the water-sucrose system. "

*5) Page 9, line 23. Please give more details on what you mean by "we have used the non-ideality described in Zobrist (2011)".*

More details have been provided, pp. 10, line 10 now reads:

"We have used the non-ideal dependence of density on water weight fraction as described in Zobrist et al. (2011) for the sucrose-water system and the ETH model, though the results above indicate that KM-GAP and Fi-PaD would produce identical profiles"

*6) The contour plots were too small for me to read. I suggest making bigger figures or figures with larger fonts.*

Contour plots were modified so that text was larger.