Title: **Technical note: An assessment of the relative contribution of the Soret effect to open water evaporation**

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Response to RC3 (Anon. Reviewer 3, 18 Sep 2024)

Review comments in italics.

Author Response in bold.

1. *This manuscript gives a detailed discussion on the relative contributions of Fickian's diffusion and Soret effect on open water evaporation, and justifies the popular practice of estimating open water evaporation through the water vapor concentration gradients. The authors prove that the Soret effect is two orders of magnitude smaller than that by concentration-dependent diffusion. I consider such kind of work is just quite rare because of limited laboratory experiments and it is valuable for us to understand the processes behind it.*

We thank the reviewer for the comments.

2. *In figure1, the thermal diffusion factor is 0.05 for N2-N2O and N2-CO2, but 0.33 for H2-CO2. It seems that even though the thermal diffusion factor of 0.33 is used for Soret effect estimation, its contribution is still much smaller compared to the concentration gradients.*

We assume the reviewer means that even if we used the (incorrect) larger value of 0.33 (instead of 0.05) the conclusions would not be materially altered. If so, we agree.

3. *In abstract, the authors mentioned that "under typical environmental conditions it is at least two orders of magnitude smaller than classical concentration-dependent mass ('Fickian') diffusion.". From Figure1 and Figure 2, we could find that the Soret effect can be neglected in open water estimation. I 'm just wandering under what conditions in open water evaporation estimation (fresh water and saline water) the Soret effect can not be ignored? If no, please add some examples in the discussion of the manuscript.*

Adopting a value of 0.05 for the thermal diffusion factor (α_T) we can pose an answer to the reviewers question about when the Soret effect might be important. The obvious condition would be where the absolute magnitude of the surface to air T difference was much larger. We assume the same data as in Table 3 (air at 298 K and 60% relative humidity, atmospheric pressure of 1 bar) but now we increase the surface temperature to 372 K (just under the boiling point of liquid water). At face value the relative magnitude of the Soret effect would be larger but in fact it is smaller than our original calculation (now 0.2 % of the total flux instead of 0.4%, based on Eqn 6a). The reason is that the surface is assumed saturated as appropriate for open water. Hence when the surface-air T difference is larger so is the gradient in mole fraction. The only way we could envisage a significant Soret effect is for the open water to be replaced by a highly concentrated brine-type solution where the saturated vapour pressure at the surface is close to zero and does not increase (much) with change in T. Again we could manufacture such a scenario but the absolute magnitude of the evaporative flux would be small. We can mention this important point in a future revision of the manuscript.

4. Further, the figure quality is not good, please make figures with good quality.

The manuscript was prepared using 'cut and paste' to insert the figures from a PDF into a Word document and we may have inadvertently used a low resolution during that process. However, for the final printed version the figures will be high quality PDFs. We will also increase the font size on the axis labels as per comments by another review (Kowalski).

5. Generally, I consider the manuscript is a good materials that can help us to understand clear the water-atmosphere interaction processes.

We thank the reviewer for the comments.