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

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Response to RC4 (Anon. Reviewer 4, 23 Sep 2024)

Review comments in italics.

Author Response in bold.

1. *This study quantifies the magnitude of the Soret effect on open water evaporation and demonstrate that it is typically two orders of magnitude smaller than the mass diffusion component (Fickian diffusion). This finding justifies the common practice of ignoring the Soret effect when describing evaporation in hydrological sciences.*

I believe this is an important study that should be accepted after minor corrections and clarifications. The manuscript is well-written and exhibits excellent readability. However, it may benefit from clarifications regarding the following points.

We thank the reviewer for the thoughtful comments.

Comments:

2. *C1: Multiple times throughout the manuscript, the sentences give an impression that evaporation is entirely a Fick's diffusion process (Line 26,212). However, estimation of evaporation also requires an explicit consideration of an energy term. Over open-water surfaces the gradient in the water-vapor is further strongly controlled by changes in temperature and incoming energy as reflected in the classical equilibrium energy partitioning approach (Slatyer and McIlroy, 1961).*

The energy balance approach (e.g. Slatyer & McIllroy 1961) is an alternative to the mass transfer approach (e.g. Fick's law) and this has been widely used in hydrology and agriculture (also see Yang & Roderick 2019, QJRMS, 145, 1118-1129). However, it has been held for more than 200 years (since Dalton's 1802 paper) that evaporation can be specified solely using a mass transfer approach.

Energetic constraints are needed in addition to (but do not replace) the mass transfer formulation for evaporation if one seeks to model the evolution of the system over time (e.g., in a land surface, atmosphere or ocean model). Here, however, we have direct experimental control of all key variables (air temperature/humidity and windspeed) and measure evaporation and surface temperature directly which avoids the need for such modelling.

3. *C2: In Line 123, it may be useful to provide a sensitivity estimate of α_T with respect to temperature using the equation from Youssef et al. (1965). This would demonstrate that variations in α_T with temperature are not substantial enough to cause significant changes in the Soret effect.*

We have done that on line 122 where we note that α_T = 0.05 (at T = 328 K, Fig. 1b) would become α_T = 0.048 at T = 300 K using the Youssef et al (1965, their Eqn 7) results.

4. *C3: In Line 135, Check the equation. Should x_a be written as a dependence on T_a as well. (x_s(T_s) + x_a(T_a))/2.*

The mole fraction in the air is specified directly by measurement (i.e., x_a) while that for the surface (x_s) is calculated at the surface temperature T_s which assumes that x_s is a direct function of T_s and this direct dependence is denoted using $x_s(T_s)$.

5. *C4: Line 147: The authors quantified the relative contribution of the Soret effect for standard conditions with data described in Table 3. Later they talk about describing the condition where mole fraction gradient would become zero and Soret effect would then be 100% of the total flux. They mention that this leads to total flux being vanishingly small "as described below (line 147)". However, the results for this condition are not described unless they are referring to the next section of the manuscript.*

We did not understand the point. The sentence starting on L147 reads; reads "However, that total flux would be vanishingly small as we show below."; so we are referring directly to the following results.

6. *C5: Line 171: It may be helpful to add a brief discussion for why the boundary layer thickness declines with wind-speed for a wider audience.*

That has been fully described in the cited reference (Lim et al 2012) and in our opinion is beyond the scope of this work.

7. *C6: One key difference between Griffani et al. (2024) and this study is the magnitude of the thermal diffusion factor, which is one order of magnitude higher in the former. While the authors provide a thoughtful justification for their use of 0.05 for the magnitude of diffusion coefficient, there is no experimental data for H2O-dry air mixtures. It would be important to validate these estimates with new experiments; perhaps the authors could include this as an outlook for future research.*

Good point. It is truly astonishing that we could not locate a single experiment involving water vapor despite a search lasting a few weeks across the libraries of the world. We can add that point in the discussion as suggested.