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

Authors: Roderick ML & Shakespeare CJ

Response to RC2 (Anon. Reviewer 2, 6 Sep 2024)

Review comments in italics.

Author Response in bold.

Roderick + Shakespeare

1. *This is an unusual and nicely scholarly piece of work which should be published, more or less as is.*

We thank the reviewer for the comments.

2. *It is interesting to discover that the Soret effect has received so little experimental investigation. In that regard, I note that philosopher Nancy Cartwright in her book How the Laws of Physics Lie uses the Soret effect as an established example of a coupled-flux process in discussing causal inference. Perhaps less established than she thought.*

As our references show, the Soret effect has been extensively investigated by physicists originally interested in developing/testing/refining the kinetic theory of gases primarily in the first half of the twentieth century. What the reviewer is referring to is that the Soret effect did not make it's way to other scientific disciplines (e.g. hydrology, atmospheric science, etc.).

3. *I have really only two comments. The first is to wonder if the authors might spend a little more time in discussing the boundary layer structure in evaporation: how does the temperature vary across it? ... Can we assume that there is local kinetic (thermal) equilibrium within the boundary layer? What are reasonable boundary layer thicknesses and temperature gradients? There is brief mention only in lines 170-173.*

As described in the manuscript, and implied by the equation, we have assumed the vapour is saturated at the liquid surface and is uniform above the boundary layer.. Between the surface and top of the boundary layer we assume a linear profile as described in detail in the cited reference (Lim et al, 2012). This was beyond the scope of the article but we reproduce Fig. 1 from Lim et al (2012) below (see Fig. R1) to show that the details are available for an interested reader. A similar profile is used to model

the temperature. Boundary layer thicknesses are also reported in Lim et al., and depend on the wind speed. For modest wind speeds, typical thicknesses are 1-3 mm as found here.
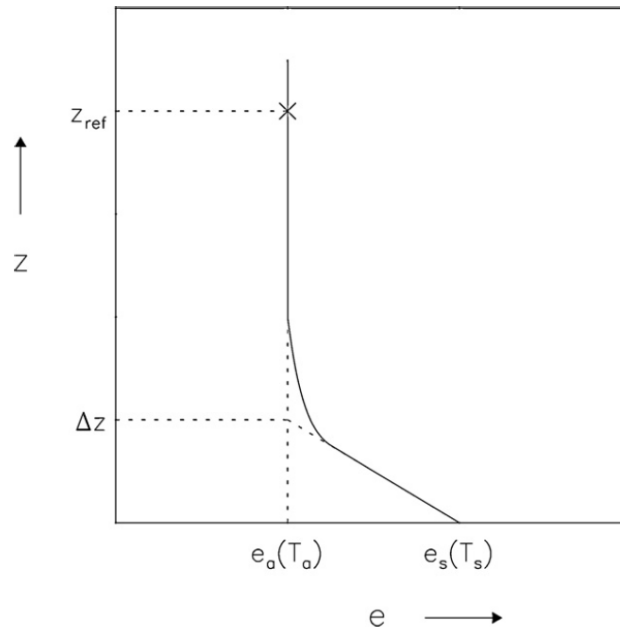
Fig. 1. A schematic diagram of the "threshold model" adopted here for the variation in vapour pressure (e) with height (z) above the evaporating surface. After Leighly (1937) and Machin (1964, 1970).

Fig. R1 Reproduced from Lim et al (2012)

4. *The second is to ask what is the connection between the the framework/ analysis set out in this paper and the description of thermal diffusion in porous media (water, liquid and vapour) originally set out by Philip and de Vries (1957) and later papers (perhaps Luikov too around the same time). Have I missed something here or should these analyses all be consistent?*

Yes, they should all be consistent.

The Philip and de Vries (1957) work was dealing with a much more complex situation with solids as well as water in liquid and gas phases. If you look at their formulation, their original equation (reproduced here) for the mass flux of water vapor through the gas phase was,

$$q_{\mathrm{vap}} = -D_{\mathrm{atm}}\nu\alpha a \nabla \rho \qquad (1)$$

Eqn 1 is actually based on Fick's Law (using vapor density $\nabla \rho$ as the driving force). In their Eqn 9 they used a classical 'Darcy' formulation for bulk flow of liquid.

No doubt one could reformulate the Philip and de Vries (1957) result in different ways but that is well beyond the scope of this paper. We note that the magnitude of thermodiffusion in soil (in either vapor or liquid phases) would be small as we have found here and could be ignored as Philip and de Vries have implicitly done.