

Review of egosphere-2024-203 by Roderick and Shakespeare

This short technical note deals with quantifying the relative contribution of the Soret effect (i.e. thermo-diffusion) compared to regular “Fickian” diffusion in the evaporation of open water bodies. The motivation behind the paper is to justify the standard practice of neglecting the Sorret effect for evaporation. For this purpose, it is based on the gas kinetic theory of Chapman and his colleagues (completed with experimental determination of the Sorret effect in binary mixtures) and on a recent dataset of evaporation rates under controlled conditions.

I think the paper is of general interest for people working of water vapor transport (even beyond the sole problem of open water evaporation) and is well suited for HESS. There is however one main point of concern (General Comment 1) that I would like the authors to clarify.

General Comments

1 – It is stated at the very start of the paper that the evaporation rate of an open water body is controlled by the ability of water vapor to diffuse in the air. From what I understand this is clearly the case when the air above the water is still (in this case, diffusion is the limiting factor, effectively setting its rate for the evaporation). However, as soon as wind and turbulence is involved, I have issues understanding to what extent the evaporation rate remains controlled by the molecular diffusion in the air.

I’m no boundary-layer scientist, but from what I understand, the diffusion flux of Eq. 4 applies within the micro layer (following the wording of Roland B. Stull’s “An introduction to Boundary Layer Meteorology”), i.e. the zone just above the surface where molecular diffusion dominates. Thus, if it is to be translated into Eq. 5 I would say that the gradients of concentration and temperature are to be taken across this micro-layer. And I’m not sure that the concentration and temperature at the top of this micro-layer can be taken as x_a and T_a (as they are influenced by the surface).

Otherwise, if x_a and T_a are taken to define the gradients, I think the diffusivity should rather be some “effective” diffusivity (including turbulent effects) and thus does not match the molecular diffusivity anymore. And in this case, it is not clear to me that one can upscale the Sorret effect to the whole boundary layer in a similar fashion (i.e. that there is an effective Sorret flux, including turbulence, that has the same form and the same thermal diffusivity ratio as in the purely molecular case).

In other words, I think the problem boils down to the difficulty of reconstructing the surface concentration and temperature gradients based on the “air” values, which I assume can be significantly different from what happen in the micro-layer.

I would thus like the authors to clarify this point. Especially, references to pre-existing literature treating this problem and relating molecular diffusion in the micro-layer to the air temperature/concentration would be appreciated.

2 – From what I understand the motivation behind this close look at the Sorret effect stems from the recent study of Griffani et al. (2024), that states that thermo-diffusion can be an effective mechanism of water vapor transport and should not always be neglected. However, this motivation only appears in the discussion. I think it could be quite beneficial to include this in the introduction, as it relates to the state-of-the-art on the subject.

Specific and technical comments

Abstract – I would systematically say “evaporation from open water” rather than simply “evaporation”.

L15 – As mentioned in the General Comment 1, references to pre-existing literature would be beneficial here.

L21 – The mention of the Duffour effect is a bit off to me, especially as it is no longer mentioned in the text. It could potentially be discussed a bit more in the Discussion and Conclusions section, notably mentioning that the Onsager reciprocal relations allows one to estimate the Duffour effect from Sorret.

L76 and L84 - I would say “limiting conditions” rather than “boundary conditions”.

L82 – If I’m not wrong the quadratic form was proposed in the work of Chapman (and certainly others). You could refer to them to justify this specific functional form.

Eq 5 - It might be just me, but I'm not fond of mixing alphabetical and digits in Equations, as it obscures physical variables from actual math constant (and I find it harder to read and interpret). I would keep α_T rather than 0.05.

L136 – It relates to General comment 1. Could you elaborate on the physical significance of Δz ? Is it the thickness of the micro-layer (where diffusion dominates)?

Eqs 6a and 6b – Same as Eq. 5. I would go straight to the point and say that Eq 5 using some standard values yields a 99.6%/0.4% partition for the Fickian and Sorret fluxes.

L149 – For me, “vanishingly small” implies that the Sorret contribution strictly goes to zero when x_s equals x_a . However, I do not think it's the case as small Sorret contribution remains non zero (if x_s is different from 0 or 1, and T_s different from T_a).

L155 – If it's the air temperature, please use T_a rather than T .

Figure 2 – I find it hard to determine the relative contribution of the Sorret flux in panel c near the origin of the graph. Perhaps add a second y-scale the relative contribution of the Sorret flux as a scatter of the total evaporation flux.

L194 – I think the wording could be improved. From what I understand, the issue is that Griffani et al.'s work is based on Landau's derivation which assumes that (i) the water vapor molecules are much lighter than the dry air and (ii) the collisions are elastic; both assumptions not applying to the actual mixture of water and dry air.

The current wording rather suggests that the issues are (i) Griffani et al. is based on Landau ($M=1$ and elastic collisions) and (ii) the collisions are assumed elastic; in which the same issue actually appears twice.

L209 – Taking α_T as 0.05 rather than 0.5 effectively reduces the Sorret flux by an order of magnitude, and the 30% contribution of Sorret in Griffani et al. now becomes a 3% contribution, which is deemed negligible. That being said, I would still leave the door open that in almost all natural conditions, the Sorret effect appears negligible, but that pathological cases (i.e. very high temperature gradients with little concentration gradients) can still exist (and it links with the paragraph below).