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Interactive comment on "Impacts of non-ideality and the thermodynamic pressure work term $p\triangle v$ on the Surface Energy Balance" *by* William J. Massman

William Massman

wmassman@fs.fed.us

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Response to Comments dated 18 June 2019 by Anonymous Referee 1

(0) My thanks to the referee for his/her comments. My numbered responses correspond to the referee's numbers.

(1) You are correct. I overlooked several formatting errors. I believe I have resolved them all.

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(2) The referee makes a very intriguing suggestion. But I cannot fully respond to it without some further study of Makarieva et al.'s hypothesis. I do see some interesting similarities, but they proceed one step further than I by coupling their "model" to an equation of motion (condensation-driven winds). This is a step beyond the intention of this paper. But thanks for drawing my attention to their work. I read the original paper, but have not had occasion to revisit it.

(3) I tried to use phrase "pure liquid water" where I thought the additional "pure" was appropriate.

(4) I have removed the sentence from the text. It was a vestige from the previous version.

(5) I will revise the manuscript to include a discussion of the internal energy and the work terms. I will also revise Figure 1 with a second axis on the right hand side with the correct scaling for $p^* \Delta v^*$.

(6) The temperature was changed to 273.16 K.

(7) (a) The referee is concerned that the initial state of the system is "non-physical". To a certain extent this is true, but this part of the manuscript is really a thought experiment and an exercise in logic designed to elucidate the relationship between non-ideal gases and the enthalpy of vaporization. In fact Equation (4) is the purpose of this exercise. Nonetheless it is possible to make the thought experiment more physically realistic by imagining an impermeable barrier separating the dry air from the liquid, which upon removal would allow evaporation to proceed along its normal course. (Of course there is no such thing as a impermeable barrier because gas

molecules would still manage to diffuse through it.) But in the final analysis this approach is identical to the one that is described in the manuscript.

(b) The referee suggests adding dry-air to a saturated liquid. This approach runs afoul of the assumption that the system is isolated (no exchange of mass or energy between the system and its surroundings). It is not possible to add dry air to an isolated system of liquid water and saturated water vapor. Therefore, the dry air must be part of the system from the beginning, in which case the total enthalpy of the initial state of the system is not $N_v h_v^* + N_l h_l^*$ because it does not include the total enthalpy of the dry air. In a laboratory setting it should be possible to add dry air to a (non-isolated) saturated liquid-vapor system. But then there would be issues of the work term associated with the expansion of dry air into the volume of saturated vapor. In turn this would raise issues concerning the diffusion of the saturated vapor into whatever volume contains the dry air. And then if the vapor pressure drops below saturation because water vapor (now no longer isolated) diffuses out of its original volume, evaporation must begin again to restore the saturated vapor pressure. Implying I now have to contend with evaporative cooling after all. I am not sure that this approach represents any improvement over what is already presented in the manuscript.

(8) I changed the symbol to ΔH_s.

(9) The referee is correct. I am referring to a process occurring at constant volume or constant pressure. Upon rereading the original sentence I see I used "The final step is to specify whether the enthalpic change occurs at **a** constant pressure or at **a** constant volume." The revision removes the two articles, "a". The sentence now reads "The final step is to specify whether the enthalpic change occurs at constant pressure or at constant volume." Sorry for the confusion. Does this address the referee's concern?

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