Reviewer #2

Review of the manuscript 'Revisiting the Thermodynamic Derivation of the Penman-Monteith Equation'.

Recommendation: Reject.
The manuscript argues that the Penman-Monteith equation cannot be applied to vegetation, based on the premise that the evaporative conditions within stomata differ from those of unvegetated surfaces. I have a few rather major concerns about this manuscript that prevent me from recommending acceptance or even a revision:
Reply: Thank you very much for reviewing this manuscript and your assessment. In the following, I will try to address your comments as best I can.

1. The author's sweeping claim is not substantiated by any comparison with observations; it is pure speculation. While it is widely recognised that there are problems with the Penman-Monteith equation (see the recent literature on the subject), the author focuses on one small aspect and does not demonstrate that this is indeed a problem or that his approach can solve it. He also makes further unsubstantiated claims, such as 'This diffusive water vapour transport [from the stomata to the air] reduces the water vapour concentration within the leaf...'. As far as I am aware, observations show that the interior air space of stomata is practically maintained at saturation. If the author knows better, then he would need to support this by references/observations.
Reply: Thanks for this comment. A diffusive net transport of water vapor from a location of higher concentration to a location of lower concentration must lead to a decrease in concentration at the location of higher concentration and an increase at the location of lower concentration. This is the fundamental principle of diffusion and has already been proven by numerous experiments. Such a decrease in water vapor concentration causes the saturation vapor pressure within the leaf to no longer be reached. This instantaneously induces further net evaporation, through which the saturation vapor pressure is restored. Since this process occurs extremely rapidly, one can generally equate the water vapor concentration within the leaf with the saturation vapor pressure. However, this does not mean that undersaturation does not occur within the leaf. If the saturation vapor pressure would always be reached within the leaf, evaporation could only occur when temperature increases. In the afternoon, when leaf temperature decreases, the plant would no longer be able to transpire. There are sufficient measurements that refute this.

This study is indeed a purely theoretical work in which I point out inconsistencies within the thermodynamic derivation. The conclusion drawn from this, that PM is not applicable to vegetated surfaces, cannot, in my view, be directly demonstrated from measurements, because no calculation method shows no deviations from observations. Therefore, from a comparison with observations, one can equally not show that the method works for vegetated surfaces. Even if the deviations from observations are small, this good result could arise for the wrong reasons. Hence, from my perspective, only a method that is physically consistent can be applied, even if it might yield good results.

2. The manuscript contains grave errors of understanding concerning the land-atmosphere system, particularly with regard to the surface energy balance, turbulent fluxes and the evaporation process. First and foremost, evaporation requires energy because this is needed to break the hydrogen bonds between water molecules. This energy is primarily supplied by the heating of the surface through solar radiation, regardless of whether the surface is vegetated. It certainly does NOT come from the sensible heat flux, as claimed in the manuscript.

Reply: The phase transition depends on the internal heat of the water. The higher this is, the more molecules exhibit a kinetic energy level that allows them to perform the work necessary to escape the attractive forces of the other molecules. I agree that this energy is provided by the sun over the course of the day. However, the energy input from the sun that increases the internal heat must also always compensate for the amount of energy required for evaporation, which reduces the internal heat of the water surface. If this energy input is lower than the evaporation induced by an undersaturation of the thin air layer next to the surface, the energy for evaporation cannot be supplied by this external energy input alone and must come from the system itself, leading to cooling. Thus, sensible heat is converted into latent heat. This should not be interpreted as a sensible heat flux from higher air layers toward the water surface, but rather as a conversion of sensible energy into latent energy within the tightly coupled system of the water surface and the very thin air layer above it, which are closely linked through molecular heat exchange. Since this apparently did not become clear when reading the manuscript, I will gladly clarify this in a revised version.

Furthermore, it is NOT the temperature difference between the surface and the atmosphere that drives the sensible heat flux because this temperature difference is not an independent driving force, but rather the result of surface energy balance partitioning in conjunction with convective boundary layer development. Much of the author's formulation reflects the common misunderstanding that originates from forcing turbulent fluxes into a diffusive form.
Reply: This manuscript does not describe turbulent heat fluxes. Here, the energy conversion of sensible heat into latent heat within the very thin air layer next to the water surface is described, as found, for example, inside a leaf. There, only molecular heat transport occurs.
In general, however, it also applies to turbulent heat transport in the atmosphere that net fluxes are always caused by potential differences (whether they are buoyancy-driven or caused by shear). Thermal energy flows from warm to cold. Solar radiation naturally provides the energy for this turbulent flux, as it warms the surface. Through the temperature difference, this energy can be released into the atmosphere. In doing so, the flux itself, of course, in turn alters the potential difference, as it warms the air and thus reduces its own driving force. Only a further increase in surface temperature can raise the potential difference again and thus also generate the turbulent flux needed to transport this energy away.

I could list many smaller points that are problematic (e.g. that most of the text describes textbook knowledge), but I will spare you these as they would distract from the most relevant points. Even if the author were able to substantiate some of his points with observations and correct some of his physics, the result would be a rather different manuscript. For this reason, the paper cannot be published and must be rejected.