Reviewer #1

The author provided a critical review of the derivation of the Penman-Monteith (PM) equation by pointing out the limited validity of the PM equation. Yet, the arguments leading to the correction conclusion are false. This research note highlights the prevailing misconceptions in evaporation physics starting long ago aggravated by Penman and Monteith through their popular evapotranspiration (ET) models.

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.

"The Penman-Monteith (PM) equation is the most commonly used method for determining latent heat fluxes (λE) from vegetated land surfaces and is regarded as a best practice for calculating evapotranspiration" (line 2-3) is not true. The author appears to be unaware of the advancement in modeling ET over the past two decades. For example, Wang et al. (2024) critically evaluated three physics-based ET models, which overcome the weakness and limitation of the PM equation.

Wang, Y., Petrone, R. M., & Kompanizare, M. (2024). Toward a unified understanding of estimating evapotranspiration: The linkage between three effective parsimonious models. Water Resources Research, 60, e2023WR036910. https://doi.org/10.1029/2023WR036910

"The resulting Penman-Monteith (PM) equation (Monteith, 1965) is today probably the most widely used method for determining latent heat fluxes from vegetated land surfaces and is regarded as the gold standard for calculating evapotranspiration" (line 100-102) reflects the view of those who overlook the progress in the theoretical and modeling study of evaporation. Numerous improved ET models have been developed. The era of Penman and Monteith has passed. The author is encouraged to read (Wang et al., 2024).

Reply: I also view the use of the PM equation for calculating evapotranspiration critically. Furthermore, I am fully aware that there are other approaches to determining evapotranspiration. The paper cited by the reviewer describes some quite interesting concepts in this context. However, the existence of these alternative calculation approaches does not change the fact that PM remains the most widely used method for calculating evapotranspiration from vegetated surfaces. The paper the reviewer referenced, for example, has only been cited twice. I therefore cannot share the reviewers view that the era of PM is over. From my perspective, it is consequently still important to engage with the concepts of this method and discuss them critically.

In this context, one must distinguish between Penman's original derivation and Monteith's thermodynamic derivation. Penman's original derivation is mathematically self-consistent and can therefore be applied and justified without the physical background that Monteith provides. However, the mathematically possible introduction of s_{Ta} raises conceptual questions, because it uses a spatial potential difference to describe a change in the thermodynamic state of air near the surface. In his derivation, Monteith provided a seemingly physically consistent explanation for this. And it is precisely this explanation that, in my view, is the reason why PM remains the most widely applied method for calculating evapotranspiration to this day. It gives an empirically-mathematically constructed method a weight that sets it apart from comparable methods.

However, if one can show that this physical derivation is not actually self-consistent and, consequently, is incorrect for vegetated surfaces, this also weakens the purely mathematical justification of PM and can help create space for alternative methods. This is the motivation of this paper and the reason why, in this paper, I follow Monteith's physical argumentation and attempt to demonstrate why this argumentation leads to incorrect results for vegetated surfaces.

I am, however, well aware that the physical concept introduced by Monteith can be very critically discussed. I believe that some points can simply be attributed to the wording and, in my view, can be reconciled. Others require a deeper physical discussion, because Monteith's original derivation leaves room for interpretation in how it defines or does not define things. In the following, I will attempt to address the points raised.

This paper contains several misconceptions about ET process including:

▪ "… higher water temperatures lead to a higher evaporation rate" (line 39). This is not true in general. Temperature is only one of the factors in ET physics. Other factors include energy supply, water supply, and boundary layer turbulence.
Reply: Thanks for this comment. This sentence refers to the section above and describes the dependence of the phase transition on the internal heat of the water. The higher this is, the more molecules transition from the liquid to the gas phase. It does not refer to the net evaporation described in the following section. To prevent further confusion, this sentence can be removed from the manuscript.

▪ "… the air is saturated with water vapor" (line 43). It is well understood that saturation vapor (pressure) is only function of vapor temperature independent of whether water vapor is mixed with air or in vacuum. Dry air is not a porous medium where it becomes saturated when all pore space is filled up with water vapor molecules. The ideal gas law tells us that water vapor is part of the (moist) air if viewed as a "porous medium".
Reply: The reviewer is right, this formulation is too colloquial. In the revised version, I would no longer speak of air that is saturated with water vapor, but rather of the saturation vapor pressure being reached.

▪ "… water concentration difference that triggers the diffusive flux drives the phase transition" (line 62), "… Evaporation can therefore be described as a transport of latent heat λE (in W m-2) that is driven by the moisture-potential difference between the saturated (air close to the) surface es and the unsaturated air ea" (line 64-65) First, phase change is energy driven phenomenon. Water concentration difference does not necessarily cause phase change, at least not the dominant driver. In the Earth environment, it is evaporation, driven predominantly by solar radiation, that drives moisture gradient near-surface instead of the opposite. Evaporation is the cause, not the effect, of the moisture-potential difference since solar radiation at land surface must be converted into latent, sensible and ground heat flux.
Reply: Phase transitions from liquid to gaseous water occur constantly; the higher the internal heat, the more water molecules transition into the gas phase. For evaporation, however, the net exchange between molecules transitioning into the gas phase and those transitioning into the liquid phase is what matters. The number of molecules entering the liquid phase depends on the water vapor concentration. When the water vapor concentration in the thin air layer in contact with the water surface is below the saturation vapor pressure, net evaporation can occur. The evaporated water molecules are then transported from this thin layer further into the atmosphere through diffusive processes. Diffusion is a process that, according to Fick's law, depends on the concentration gradient.
This relationship can be most clearly illustrated using transpiration. There, the phase transition occurs inside the leaf. Through the opening of the stomata, water vapor diffuses into the surrounding air, which causes the water vapor concentration inside the leaf to fall below the saturation vapor pressure and induces further net evaporation. Importantly, this does not refer to the moisture concentration at a height of 2 meters, since no diffusive processes occur there, but rather to the concentration in the air mass adjacent to the leaf (This is not clearly defined in Monteith's derivation). The intensity of the diffusion process thus affects the net phase transition. This is the reason why moisture differences in the air lead to further evaporation for a given internal heat of the surface. The internal heat of the water itself, of course, depends on the energy input from radiation. The evaporation caused by this energy input and the diffusive transport of water vapor molecules out of the leaf then increases the water vapor concentration near the ground, creating a moisture gradient into the atmosphere, which, through transport processes, enables a net transport of water vapor into the atmosphere.

▪ “The air supplies this energy in the form of sensible heat. The surface takes this sensible heat, which raises the kinetic-energy level of the water molecules, causing them to transition into the gas phase more frequently. As a result, the latent heat content of the air increases. In other words, the air transports energy in the form of sensible heat to the surface and, in return, receives an equal amount of energy as latent heat” (line 188-191) This is one of the most nonsensical arguments of Monteith in deriving the PM equation. Downward sensible heat flux as the energy supply of evaporation rarely occurs, if at all, under natural conditions especially during daytime. Although downward sensible heat flux may occur over the oasis surface as a secondary energy supply to enhance evaporation, solar radiation remains the dominant driver of evaporation. I am not aware of any observations confirming downward sensible heat flux is the primary energy supply of evaporation.

Reply: I admit that the use of the word "transport" in the last sentence of the citation is unfortunate and again too colloquial. In principle, the point here is that the evaporation process requires energy, and evaporation therefore leads to cooling at the evaporating surface. I think this phenomenon is undisputed, as it is the basis of air humidity measurement using a psychrometer. That evaporation requires energy is always the case, whether energy is supplied by the sun or when no energy input occurs. In the latter case, energy is extracted from the system and converted into latent energy, so that the surface cools and latent energy is transported into the air. Monteith considers the thin air layer in direct contact with the liquid phase (not higher air layers) together with the surface as a closed adiabatic system. Although Monteith does not define exactly which air layer he means in his derivation, which creates room for interpretation, the physical relationships he describes can only be explained for this thin air layer.

In such an adiabatic system, the energy for the phase transition is, by definition, provided from the system itself. I consider this notion quite justified, as it emphasizes how strongly the liquid phase and the thin air layer are connected through molecular heat exchange. Thus, if net evaporation leads to energy extraction and cooling of the surface, this also affects the adjacent air layer (and the water vapor contained therein) through molecular heat exchange. This layer also cools and releases energy to the surface (which also reduces the saturation vapor pressure of the water vapor). If there is an external energy input such as solar radiation, this energy input must always also compensate for the cooling by evaporation, even if the surface warms up.

▪ “… air mass rises, … its capacity to hold water vapor” (line 219), “the capacity of the air mass to hold water vapor” (line 319-320) This is a widespread misconception about water vapor repeated several times in this paper. As explained above, the air is not a porous medium like soil and does not have capacity to hold water vapor.

Reply: As already described, the point here is whether the saturation vapor pressure is reached or not. The wording would be specified in a revised version.

▪ “… the air mass where evaporation occurs” (line 335) Evaporation, i.e. the phase change of liquid water over soils and in plants, does not occur in the air as phase change always occurs at liquid-vapor interface. Certainly, this paper does not discuss rain drops evaporate as they travel toward surface as rainfall.

Reply: Thanks for this comment. I agree with the reviewer. This formulation is also too colloquial. In the revised version, I will speak of a "thin air layer in contact with the surface."

▪ “… the spatial temperature difference between the surface and the near-surface air, which drives the sensible heat flux” (line 408-409) Under natural conditions, temperature gradient is driven by sensible heat flux for the same reason that humidity (moisture-potential) gradient is driven by vapor flux since solar radiation must be converted into thermal energy fluxes at the land surface.

Reply: Fluxes are always driven by potential differences. This applies in hydrodynamics, where a flux moves liquids or gases from high to low pressure (Hagen-Poiseuille law), but also to electrical fluxes caused by electrical potential differences (voltage) (Ohm's law). According to this concept, a sensible heat flux is likewise caused by temperature differences (energy flows from warm to cold (Fourier's

law)). Solar radiation naturally provides the energy for this 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 flux needed to transport this energy away.

The two terms in the PM equation, commonly referred to as available energy (diabatic component) and atmospheric demand (adiabatic component), are mathematical artefact as the result of flux-gradient formula of vapor and heat flux and the approximate expression of surface temperature in terms of air temperature according to the Clausius-Clapeyron equation using the first order Taylor expansion. It is important to realize that Eq. (1) is empirical instead of based on physical principles. Decomposition of $es - ea = (es - ed) + (ed - ea)$ in Eq. (7) by introducing ed, leading to the expressions of diabatic and adiabatic component of EG in Eq. (12), is totally arbitrary, hence a mathematical artefact. In fact, $E1$, $E2$ are physically unreal and coupled mathematically. It is impossible to change one arbitrarily while fixing the other. In addition, G in Eq. (8) cannot be arbitrarily fixed while Ts (and Ta) changes. Just because the Penman equation has two components does not mean there are two separate evaporation processes.
Reply: I believe the main misunderstanding here is that the adiabatic and diabatic components are understood as independent processes that occur completely sequentially and would have to be observed and measured as such in reality. However, I would interpret these two components as the partial vectors of a kind of "ET vector" for a steady state. Thus, it is not that one of the two components is fixed while the other changes; rather, only the partial vectors of the evaporation vector are described. Consider, for example, such a steady state: the solar energy input increases the internal heat of the liquid water, causing more water molecules to transition into the gas phase. At the same time, the release of sensible heat also raises the temperature of the adjacent air layer and thus also of the water vapor contained therein, which increases the saturation vapor pressure and allows a net transition of water molecules into the air (diabatic component). The resulting increased water vapor concentration enhances the diffusive transport of water vapor into the surrounding air, causing the vapor pressure to fall below the saturation vapor pressure again. This undersaturation instantaneously leads to further net evaporation that requires energy and draws it from the previously warmed system (adiabatic component). In a steady state, the temperature does not change at all through these processes; they occur simultaneously. An increase in radiation then alters this equilibrium, leads to a higher temperature and higher evaporation, and thereby to a new steady state to which PM can be applied.

Monteith's thermodynamic derivation of the PM based on the assumption of the existence of two evaporation processes is based on observational fact and the diabatic and adiabatic component are not real physical processes. Monteith's interpretation of the diabatic and adiabatic component is based on imaginary processes, hence is misleading. Essentially, Monteith justified the mathematical artefact using false physical arguments to fit the Penman's formula. The thermodynamic derivation of the PM equation by Monteith is built on a series of misconceptions and unrealistic assumptions about ET process. Monteith justified the two terms in Penman's equation by associating them with unreal physical processes: diabatic and adiabatic evaporation. Monteith's questionable arguments appear to root in the misconception of evaporation occurring in the air. "For the description of the adiabatic component of evaporation E_1, the air mass into which evaporation takes place and its contact with the surface are initially defined as a closed, adiabatic system that exchanges no energy with the surrounding environment" (line 176-179) indicates E1 is totally imaginary resulting from non-existent physical processes. "The air supplies this energy in the form of sensible heat. The surface takes this sensible heat, which raises the kinetic-energy level of the water molecules, causing them to transition into the gas phase more frequently" (line 188-189) is not what happens in a natural environment. During daytime, surface temperature increases due to solar radiation as the energy supply for evaporation, sensible heat flux into the air and ground heat flux into soil. Therefore, Eq. (13) is physically meaningless. Nighttime sensible het flux over land surface is indeed downward from air to surface, but nighttime evaporation is usually negligibly small if at all. Instead, condensation in the form of dew is a

common occurrence that releases latent heat to partially balance longwave radiation loss. Since the decomposition of evaporation into diabatic (available energy) and adiabatic (atmospheric demand) component in Penman's equation is a mathematical artefact, it is not surprising that they are not associated with real physical processes. Monteith's thermodynamic derivation of the PM equation does not make physical sense. Even worse, Monteith's work has misled generations of hydrologists by spreading several misconceptions about evaporation physics.
Reply: Please see the previous answer.

The author's argument following those of Monteith to derive the PM equation for vegetated surface in Eq. (31) is also based on the same misconceptions including evaporation driven by moisture-potential difference. Without energy balance constraint, the claimed correct PM equation for vegetated surface goes back to pre-Penman models that Penman and Monteith proposed to improve. Therefore, the conclusion "the Penman-Monteith equation is not applicable to vegetated surfaces" (line 433) is partially correct but based on false premises. The claim "The thermodynamic derivation of the PM equation is therefore internally consistent" (line 396-398) is correct but should be understood as "consistently unphysical and misleading". This theoretical work would be useful if the author could provide an in-depth discussion of these common misconceptions about evaporation.
Reply: As already mentioned in the introduction, I consider it important to show that the physical justification for the mathematically consistent introduction of s_{Ta} is not valid for vegetated surfaces. Only in this way can the mathematically consistent PM equation be called into question for general application. In this context, I also do not agree with the reviewer that it was the intention of Penman and Monteith to improve methods based on moisture differences. The development of PM was solely due to the fact that measurements of these moisture differences between the surface and the atmosphere were simply not available, but only atmospheric measurements, and thus other ways had to be found to calculate evapotranspiration without having this information.

The paper contains too many equations (total 31). Some of them may be put in an appendix or "Variables and Symbols".
Reply: Since this manuscript aims to demonstrate the inconsistency within the thermodynamic derivation of the PM equation, it is, from my perspective, important to first describe this derivation in order to make its inconsistency logically comprehensible.