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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential evaporation trends over land between 1983–2008: driven by radiative or turbulent fluxes?" *by* C. Matsoukas et al.

Anonymous Referee #1

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Journal: ACP Title: Potential evaporation trends over land between 1983-2008: Driven by radiative or turbulent fluxes? Author(s): C. Matsoukas et al. MS No.: acp-2011-108 MS Type: Research Article Iteration: Initial Submission

The paper's introduction describes how pan evaporation values have been decreasing globally with only a few exceptions, such as in central Australia. The paper then goes on to calculate potential evaporation from Penman's equation, using a radiative-transfer model and ISCCP data to calculate the net radiative flux into the surface (E_r) and reanalysis winds and relative humidity to calculate the aerodynamic flux (E_a, which uses the saturation humidity of the air instead of the surface). The paper calculates

a result that is the exact opposite of the observations: the conclusion is that potential evaporation should be increasing in most places, except, for example, in Australia, where it should be decreasing. It is argued that this should be driven by increases in net radiative fluxes into the surface (as opposed to changes in the vapor pressure deficit of surface air). But, no attempt is made to reconcile these conclusions with the observations.

Below are my comments and recommendations.

* Misleading title

Penman's equation gives an expression for the latent-heat flux in terms of the net radiative flux into the surface and the vapor pressure deficit in the surface air. Since these are the two factors being analyzed here, the title of the paper should be something along the lines of "Driven by radiative fluxes or vapor-pressure deficit?" The choice between radiative and turbulent fluxes presented in the current title is a false choice: radiation supplies the energy that the turbulent fluxes carry away.

* The text misrepresents the meaning of Penman's equation.

I do not disagree with the paper's use of Penman's equation. It is written correctly in equation 6, which gives the potential evaporation in terms of a weighted average of E_r (the net radiative flux into the surface) and E_a (the aerodynamic flux of latent heat that would occur if the skin temperature were the same as the surface air temperature). But, the meaning of Penman's equation is misrepresented by the text. Neither E_r nor E_a are useful models of the latent heat flux, but the manuscript refers to them incorrectly that way: "The aerodynamically derived evaporation E_a is one model of the evaporative process" and "the energy balance evaporation rate E_r... is E_r = R / rho L," where R is the net radiative flux into the surface. The text then refers to Penman's equation as a "method" for combining the two methods. But, Penman's equation is not a method. It is an equation that can be derived using three very good approximations: an aerodynamic formula for sensible heat (the right coefficient must be known), an aerodynamic formula for latent heat (again, coefficient must be known), and a first-order Taylor expansion for saturation vapor pressure.

* Confusing paragraph

I was not able to follow the paragraph starting on line 32. It should be rewritten for logic and clarity.

* Longwave emission from the surface

In section 2.1.2, an explanation is needed of how the upward longwave from the surface is treated. Is the skin temperature set to be the same as the reanalysis surface air temperature? How much error does this introduce in the upward longwave flux? Figure 2b shows that the Sahara, the Arabian Peninsula, and Australia have large values of E_a/E_r, which implies large negative sensible heat fluxes. Those large negative sensible fluxes imply that the skin temperature is much less than the air temperature. If the surface air temperature is being used for the longwave flux, this would make the longwave fluxes biased. How important is this effect?

If some other method is being used to estimate the skin temperature, what method is that? Some explanation is needed.

* Stability and the aerodynamic formulae

The stability and instability of the boundary layer changes the coefficient of the aerodynamic formulae quite significantly in reality. This would be especially true for the strong inversion over the theoretical bodies of water over the desert regions where the Bowen ratio is large and negative. How can changes in stability be discounted as an important driver in potential evaporation rates?

* Figure 2c does not make sense to me

Penman's equation gives the potential evaporation E_p as a weighted average of E_r and E_a. Since the weights are positive, E_p must be a value between E_r and E_a.

Therefore, all of the values in Figure 2c (E_p) should be between the values displayed in Figures 1a (E_a) and 2a (E_r). In Figure 2c, this is clearly not the case. See, for example, the Sahara.

* Why not use reanalysis for the calculation of E_r?

What is the advantage of calculating the radiative fluxes from scratch using ISCCP and a radiation code? Why not use the reanalysis, which I presume has already done this job?

* Why not compare potential evaporation from this analysis to the reanalysis?

In Figure 6, a comparison is made of the anomalies (normalized by the respective standard deviations) of the global E_p between the method presented in this manuscript and two ERA products. It would be more helpful to see how they compare in absolute terms and on a regional basis. Please include a figure showing a global map of annually averaged E_p calculated using the three methods (manuscript, ERA-40, and ERA Interim).

* Results contradict observations

The introduction explained in great detail how pan evaporation rates have decreased. It was a surprise, then, that the manuscript did not give any explanation for the opposite prediction obtained with Penman's equation. Figure 11 predicts an increase in pan evaporation rates almost everywhere. One of the exceptions is in Australia, where the manuscript concludes that pan evaporation rates should have decreased. But this is one of the few places where, as stated in the introduction, pan evaporation rates actually increased. So, these postdictions seem to be the complete opposite of what was actually observed. Does this invalidate the method? What went wrong? Or what is wrong with the observations?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10935, 2011.