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Hydrology and Earth System Sciences
Discussions

Interactive comment on “Relative humidity gradients as a key constraint on terrestrial water and energy fluxes” *by* Yeonuk Kim et al.

Anonymous Referee #1

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This interesting paper explores a formulation of ET fully independent of the surface resistance, relying instead on a humidity resistance depending on the gradient of moisture between the surface and screen level, following the ideas displayed in Monteith (1981). Conceptually this approach allows to skip any explicit dependency of the characteristics of the surface, particularly those of the vegetation. The approach is worth exploring and this manuscript intends to show us to what point it can be used, especially for interpretation of the processes in place, more than in a parameterisation mode.

In a set of recent papers McColl and colleagues showed that, for scales of a day or longer, ET is essentially determined by the balance between surface moistening and surface heating, with the consequence at these temporal scales that more atmospheric

moisture is the result of more ET, and the term describing it is equivalent to the diabatic term in the Penman-Monteith equation, therefore independent of the characteristics of the surface.

In this manuscript, the authors place themselves in this framework and try to extend it to sub-daily scale by using the humidity resistance in the adiabatic term, avoiding to prescribe any characteristic of the surface (vegetated or not). Their theoretical reasoning is easy to follow, substituting the pressure vapour by the relative humidity, and it is intended to be valid even for non-saturated surfaces through the prescription of a "surface relative humidity", rhs.

The authors do not explicitly comment or define rhs, which is still the missing piece in most of the approaches dealing with non-saturated surfaces. In the present case, they go around the problem of defining or calculating rhs by deriving a value for it by using observed LE and H values from EC-systems, using their formulation (2).

To do it they assume that the available energy Q is LE+H instead of Rn-G trying to circumvent the unavoidable problem of the closure of the surface energy budget. This decision could be understandable but it is poorly justified and the consequences of it are not reflected upon.

Unfortunately the result of these strong hypotheses concerning rhs and a discussion of the values obtained is not explicitly shown or made.

The proposed method cannot be used as an independent way to determine LE, because LE is used to determine rhs, so obtaining again LE from this rhs value would be of no use (unless I miss something). However the method is useful to separate the observed LE into its diabatic and adiabatic parts (taking into account that Q=H+LE is a tough assumption), and this is where most of the interpretation effort is put on.

When analyzing data from a site in Costa Rica it is shown that for daily values (and larger temporal scales) the diabatic part explains most of the total LE, the adiabatic part being most significant in dry and low-vegetated conditions. At the sub-daily scale, the adiabatic part has a opposite behaviour to the diabatic one, which I interpret as turbulence tending to bring vapour towards the surface layer in the daytime, and removing it in the nighttime and early morning.

Fig 4 contains a lot of information in its 8 sub-figures, which are not really commented. A similar comment can be made about Fig 5 and its 12 sub-figures. What is the use of displaying so much information if then it is not discussed? In general section 4 would need to be more developed in terms of interpretation of results, which is now very shallow, especially sections 4.2 and 4.3.

As the paper is now sections 5 "Discussion" and 6 "Conclusions", both very short, could be merged into one larger Conclusions section. Instead a real "Discussion" section could come from an expanded version of the analysis of the results.

So I suggest to the authors that they i) clarify somehow the aims of their research in the initial parts of the paper, especially in the abstract, when one may get the impression that a new ET parameterization is presented, while in reality what we have is a nice method of analysis of the diabatic and adiabatic components of ET; ii) elaborate on the meaning of rhs; iii) explain better what are the expected consequences of their hypotheses in the ulterior data analysis (such as imposing Q=H+LE or the chosen form for ra); iv) expand they interpretation of data, currently very shallow, into a re-made Discussion section; v) joint the current "Discussion" and "Conclusions" sections into a more comprehensive and developed new "Conclusions" section; vi) consider to summarise the information in the supplementary material and incorporate it straight into the manuscript

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