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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of evapotranspiration from TOA radiances in the Poyang Lake Basin, China" *by* J. Peng et al.

C. Jimenez (Referee)

carlos.jimenez@obspm.fr

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The paper presents evapotranspiration estimates based on the "triangle method" using top-of-the-atmosphere (TOA) radiance and TOA NDVI, instead of the more usual land surface temperature (LST) and NDVI (at the surface). The paper is well written, their contents are well presented, figures and table are clear, and the subject is of interest for HESSD readers.

My main points of concern are related to

(1) Surely atmospheric corrections introduce complexity and uncertainty into remotely sensed variables at the surface. But there are required to avoid the signal retrieved

at the surface being modulated by the atmosphere. In my opinion a surface-variable-triangle-method and a TOA-variable-triangle-method working similarly is due to the fact that estimation errors associated to this specific methodology do not allow to distinguish between the use of one or another type of algorithm forcing. If TOA variables (instead of surface variables) were best suited to address a surface retrieval problem, that would indicate a very poor atmospheric correction (uncertainty from atmospheric correction larger than atmospheric signal modulation). That could be the case over a specific place or time of the year, but it is difficult to believe that this would always be the case. It should be noted that for some of the variables discussed other factors may be at least as important (in terms of adding uncertainty) as the atmospheric correction (e.g. land emissivity for the derivation of LST, see for instance Jimenez et al., 2011). For this estimation, using a TOA thermal emission instead of a LST neglects the modulation in the thermal signal by the emissivity, which may (or may not be) of importance here. So I understand that TOA variables are used for the sake of simplicity as it does not matter for the end result in this particular case (ET estimation), but I would not say that we are using less uncertain remote sensing observations as the authors seem to conclude.

(2) The validation of the proposed methodology is carefully done, but very limited in time and space. Statistics are computed just over 16 case days for just one location. Giving the phrasing used in the text, the paper seems to indicate cloud cover as the limiting factor for case selection. Even if this is a clear sky day technique, selecting 16 cases from a 9 months period over a sub-tropical area of $\sim$20,000 km2 casts serious doubts about the applicability of this technique. For the MODIS TOA and MODIS product comparison this is further reduced to 11 cases. Giving that the difference in the statistical figures are not large for many of the comparisons, conclusions about one methods and/or dataset being better than other should be very carefully drawn. The paper would clearly benefit for more solid statistics (in terms of number of cases, not methodology).

(3) Looking at the statistics comparing TOA ET and surface-variable ET, the differences

do not seem related to the triangle-method itself (both EF seems quite similar), but rather to the net radiation (poorer for the surface-variable formulation, compared with the TOA formulation). So the differences are more related to one of the ET algorithm forcings, rather than the algorithms themselves. Without being an expert, I assume that there are different schemes to estimate Rn from the set of MODIS variables, not just Bisht 2005, which may (or may not) give better results for this specific location. Granted that net radiation is an important part of an ET estimation, but it seems to me that we are discussing (for this specific location and the 11 days compared) net radiation algorithm issues (more than ET estimations).

(4) Giving that the paper mainly uses MODIS products, it could have been made more interesting by including the MODIS ET official product (MOD16, Mu et al., 2011). This is not a ground validation as the use of a weighting lysimeter, but it would allow to compare this triangle-method with other ET estimates from the same instrument, and to discuss this work at larger scales and in the context of one of the main methodologies (the Penman-Monteith based approaches) use at the moment for regional/continental ET estimation from space.

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