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Hydrology and Earth System Sciences Discussions

Interactive comment on "Temporal variations of evapotranspiration: reconstruction using instantaneous satellite measurements in the thermal infra red domain" *by* E. Delogu et al.

Anonymous Referee #3

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The question of how to best fill gaps in seasonal ET retrievals using thermal remote sensing is an important one, and this paper tests two standard approaches that are commonly used - the "stress factor" approach (conserving the ratio of actual to some reference ET) and conservation of "evaporative fraction". Understanding the relative merits and limitations of these approaches under a broad range of climate and vegetation conditions, and for different satellite revisit frequencies, is critical to improving TIR-based ET applications and planning future ET-based satellite missions.

Stylistically, the paper started out well with a good review of the literature and clearly setting up the problem. It became increasingly wordy and disorganized as it progressed

into the results. Revision for clarity, organization, and grammar is required.

Of most concern, however, is that some non-standard SF and EF approaches were applied and that may have significantly influenced the outcomes. The SF results are so poor in Fig 5 in comparison with typical seasonal retrievals using SF methods that I wonder if there was an error in the computation. One would expect the simulated and observed fluxes to reconverge every few days whenever there was a clear-sky acquisition (since it was assumed that the instantaneous fluxes were perfectly retrieved).

It might have been better to test some standard techniques using these great datasets, so that it is easier to distinguish errors due to missing data from errors in methodological assumptions.

Why not test EF first using the observed RN and G to define daily AE, then compare this "best case" scenario with results obtained using the simplified AEd approach?

For SF, why not test an approach like that used by R. Allen in METRIC, where the scaling flux is a standard reference ET for grass or alfalfa? It is not clear why a detailed energy balance model is required to specify the scaling PET... RefET is a much more accessible datastream (less demanding than EF methods), and does not require much ancillary input. There are other simplified RefET methods like Makkink promoted by Henk de Bruin. Or a comparison between simple scaling fluxes (RefET, Priestley-Taylor, Makkink) could have been performed. This would also allow an assessment of when it is important to account for advective components in the scaling flux, or whether a PT method typically suffices. It is not clear to me that the results presented here are not specific to the choice of PET method, rather than the merits of the SF approach in general.

Specific suggestions:

P1701 L4: Change "spills" to "spells"

P1702 L9: Change "space" to "spatial"

L17: “Monitor” to “monitoring”

P1703: Suggest adding “can in turn be resolved using geostationary satellite data and/or standard meteorological observations”. Not sure it’s that easy to do rigorously, however.

P1704 L8: Change to “These authors showed that the assumption. . .”

P1706 L5: Change to “One must note that, unlike the Evaporative Fraction,. . .”

P1707 L15-16: Ratm requires *some* ground data (or met. analysis fields)

P1708 L22: Change to “South Eastern”

P1709 L16++: First sentence reads awkwardly. Maybe “. . .in the TIR domain, the only waveband that can provide soil moisture status at field scale from a spaceborne platform.” Microwave has an advantage in the time scale department (at least for polar orbiters), so would remove that comment here. . .

P1710 L25: Interesting that the clear sky #s don’t decrease between 10AM and 2PM, contrary to what is typically held to be anecdotally true. It would be interesting to expand on this study and that of Lagouarde using global flux datasets, under different climatic conditions.

P1711 L10: Awkward – maybe “is observed well after a rain or other water input event (i.e. irrigation) and ends with the next water input event.”

L20: “..used parametric equations to derive daily. . .”

P1712 L1-14: The validity of Eq 4 should be tested with the flux datasets. The simplicity of this formulation may negatively influence the final results.

L22: “remote-sensing information.”

Section 2.5.2: Why not test a more standard scaling flux that is more accessible to the wider public – like the reference ET for a grass surface, since that is what is most

commonly used when a stress factor approach is applied?

L1713 L23: This statement is unclear – the data needed to compute PET or RefET can be measured in situ. . .

L1713: In general, the EF method requires more info than a SF method (need to constrain both RN and G, whereas SF requires only a refET estimate). This statement only applies to the specific configuration tested in this paper, but typically the data demands are the other way around. Typical applications of the EF method requires some met data (at least air temp) to get daily RN and G during cloudy days when TRAD is unavailable. The next statement on pg 1714 is also atypical – SF methods generally use only a standard description of PET like Priestley-Taylor or PM, and do not use an energy balance method to get PET.

P1718 L23-25: This sentence needs to be rewritten.

P1720: In this test, did you assume a set satellite revisit frequency, but then eliminate those days when it was not clear on that revisit date? The sampling interval will typically be much longer than the revisit interval. Make sure this point is clearly made.

P1721: Indeed this is not the best way to evaluate optimal revisit frequency. For many applications, not only seasonal but daily ET/stress is important. Why not demonstrate the impact of revisit frequency on both daily and seasonal errors? I think you would see a different structure at the daily timestep.

Eq 5: At what height is RH measured in this equation.

Fig 3: Vertical axis label should read “simulated”

Fig 4: Caption and axis labels are insufficient to understand content of this figure

Fig 5: Caption and labels should read “simulated”. I don’t understand how the results for SF are so bad if the observed and simulated ET are pegged each clear day to be equal at the assumed observation time. Are you sure there is not a computational problem? Indicating somewhere on this plot which days are clear and used for pegging the interpolation would be useful. Unless the time period between clear days is very long, this is atypically bad performance for an SF method. (Why does simulated ETd drop to near zero right after the beginning of the simulation?)

Fig 6: Caption and axis labels are insufficient to understand content of this figure. Plot annotations are too small to read.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1699, 2012.