

Interactive comment on “Microwave implementation of two-source energy balance approach for estimating evapotranspiration” by Thomas R. Holmes et al.

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General comments

The paper presents an application of the ET modelling framework ALEXIS where the original TIR data is replaced by MW data. The performance of the new ALEXIS estimates are evaluated by comparison with the TIR based estimates, and by comparison with latent heat fluxes from a selection of tower sites. Overall, the paper is well written, the analysis of the presented results is adequate, and the figures illustrate well the major points of the analysis.

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Intentionally, even if this is a MW based product, the authors have limited the analysis to the clear-sky estimates to facilitate the evaluation with the TIR estimates. I agree with this rationale, but I think the opportunity to already introduce some all-sky estimates in the tower evaluation was there. I suppose that even if there are some doubts about the boundary layer modelling for cloudy conditions, ALEXIS has been run for all available MW-LST Trads, so the ET outputs were available. If so, it would have been very interesting to see a tower evaluation of the 7-day totals of clear-sky ALEXI-TIR and ALEXI-MW, together with and ALEXI –MW 7-day totals but using all MW-Trad available. Presumably, estimating ET without the clear-sky restriction would have resulted in a better match to the tower observations, justifying all the trouble of developing a MW-based product. Given that using the MW Trad reduces the spatial resolution of the ET estimates by a factor of 5, there has to be some gain somewhere to justify their use.

I find adequate the level of description of ALEXIs in the text, but I'm missing some info about the concrete input datasets for this version of ALEXIS. Even if a paper presumably dealing with that is in review and cited in the text, at least the basic datasets to run ALEXIS in this global version should be given. The meteorology plays a crucial role in the derivation of the ALEXIS output, so I imagine that compared with the previous CONUS applications, something has been changed in that respect. A critical parameter there is the surface radiation, as the LST is also involved, and it may be of interest to know how the new LST data has been handle in that respect.

Specific comments

P1. L11. Given our current inability to properly validate global ET, I doubt that this exercise can be used to validate whether your diurnal temperature information from the MW obs is correct. You should show that your LST is reasonable before you attempt to estimate ET, as I think you did in previous publications, in which case there are no reasons for the ET to be unreasonable.

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P1. L25. The limitation to clear-sky estimates applies to those ET methodologies relying in TIR data, but there are already all-sky global ET estimates not depending that clearly on TIR data, perhaps “all-sky LST-based estimates”?

P1. L28. The abstract should give information about the geographical coverage of the exercise, global?

P3. L11. Most ALEXI applications have been restricted in the past to the CONUS. Now, that it is starting to reach other domains, I wonder if there are some thoughts regarding an estimation of rain interception by the canopy, which can have some importance at some regions.

P3. L26. I think a few lines about the global implementation of ALEXI should be written. It is not clear if the global ALEXI is just plain ALEXI with global inputs, or something else has been changed. Also, what inputs ALEXI uses and the specific datasets are always interesting information. There are regions where Trad will have a very small impact on the ET estimates, i.e., there will be mainly driven by the meteorological data, so it is relevant to know about that data as well.

P3. L38. Is then the longwave heat loss recalculated with each temperature dataset? If so, how? I guess you need a daily integrated value for the Rnet, which is not straightforward as the LST datasets are not geostationary this time.

P4. L6. And when not over the GOES disk, how is MODIS LST converted to a Delta-Trad?

P6. L1. It is not clear where the 7-day total comes from. I imagine there is a reason, linked to MODIS sampling (the MODIS ET product is 8-days). The meteorology can change quite significantly in 7 days, so if the 7-day total is based on one-day sampling, I can imagine that for some conditions the errors introduced can be large.

P6. L3. So the solar radiation is daily, and the ratio of ET to the radiation maintained constant for the 7-day period?

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P6. L11. What is the reason for the missing 31% MW estimates when there are TIR estimates? The spatial sampling of the MW product?

P6. L12. Why should the MW-LST being calibrated to MODIS-LST?

P6. L25. Do you use the energy closure corrected fluxes or the original ones?

P6. L30. This is a bit confusing, even if discussed already in Section 2.3. If only in Africa and Europe the scaling of MW-LST supported, but you have then MW-LST globally, and presumably the ET, how “bad” is the MW-LST and ET outside those regions? Or, in other words, can we globally use the MW-LST ET or we need to wait for further developments in the MW-LST calibration outside those regions?

P7. L15 14 mm/month) THAN? Then?

P7. L18. I guess that's the grey color in Figure 4. It should be mentioned in the figure caption.

P8. L1. Have you found any explanations for the lack of agreement in the Horn of Africa? Trad seems to agree relatively well.

P8. L32. I'm surprised by a large part of France being masked by the grey color, i.e., not showing inter-annual variability. Most of the other grey areas are in the very dry regions, as expected, which is not the case of France.

P9. L14. Yes, very counterintuitive result and a bit worrying regarding the quality of these metrics. What do you mean by spatially uncorrelated noise? What MODIS? LST or the ET? This is not clear to me, it may be worth explaining better if you think you know the causes of this behavior.

P9. L20. What about the GOES-based time-extrapolation of the MODIS-LST to obtain Trad, can we see something here?

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P9. L24. Water limited regions are where Trad has a larger impact on the ET estimates. This suggests that ALEXI operates better when the ET depends less on Trad, right?

P10. L13. I think this needs a bit more of elaboration. Why do we want to merge both estimates? What is the overall strategy? The landscape is something that we cannot change, do you mean merging only when the landscape is homogeneous? But surely, merging spatial estimates at different resolutions will require up-scaling of the finest resolution one, or am I missing something?

P10. L15. I think is a bit too simplistic to just blame signal to noise ratio in arid regions as the reason for the poorer performance of ALEXIS there. Deriving the accurate stress needed for the water limited regions is where current ET methodologies, independent of modeling framework, tend to struggle, and ALEXI does not seem to be an exception here.

P11. L15. Why is this result surprising? If the MW-LST and TIR-LST are reasonable estimations of the true LST, and the rest of the ALEXIS modeling and inputs are unchanged, I will not expect large differences between both ALEXIs.

P11. L24. I would agree if ALEXI were using raw MW-LST and TIR-LST observations. But the MW-LST has been fitted to the TIR-LST, so I am not sure the independence is that obvious as stated in the text.

P11. L27. The improvement is 0.02, very modest, and it is obtained by combining estimates of different resolution, with one of them covering an area 5 times larger than the other. Given the discussion of the spatial degradation helping to improve correlations, I'm not sure we are just dealing with independence of datasets.

P11. L30. So far, all this is clear-sky. I suppose that MW-LST will be used always, not only for clear-sky, and that some work may be needed to see whether the assumptions of boundary layer development work for all-weather conditions. If so, it may be worth adding to this short roadmap.

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