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Reply on RC2

Zhaofei Liu

Author comment on "The accuracy of temporal upscaling of instantaneous evapotranspiration to daily values with seven upscaling methods" by Zhaofei Liu, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-73-AC2, 2021

Response

Ms. Ref. No.: hess-2021-73

Revised title: Accuracy of temporal upscaling instantaneous evapotranspiration in simulating daily values in remote sensing applications

Author(s): *Zhaofei Liu*

It would be greatly appreciated for your kind reviewing to this paper. Thanks very much for your valuable comments and suggestion. For your convenience to re-review the paper, the response corresponding to your comments are described in detail as follows:

The author studied 7 different methods to upscale instantanuous remotely sensed evaporation to the daily scale under different weather conditions. The manuscript is well structured, easy to read, and in good English. The paper is relevant and suitable for the HESS-audience and provides new insights. Most of my comments are minor (see below), but two points may need further explanation:

1) In the introduction it is stated that there already exist many (review) studies which focussed on this topic. Hence my question: how does this paper differ from those? What is the problem statement of this study? Is it that you investigate the topic also under non-clear sky conditions, plus the fact that you investigate the effect of 'time of satellite overpass'? If so, please elaborate on this and maybe refocus your study.

Reply: As mentioned in the fifth paragraph of the introduction (P3 L75-91), there are several studies focused on the evaluation of different upscaling methods. However, “In general, previous research has largely evaluated upscaling methods on a regional scale” (P3 L91-92). In addition, these studies have obtained different optimal methods at regional scales.

As described in P3 L83-85, “Based on 126 FLUXNET global sites, Wandera et al. (2017) evaluated three EF methods (Rs, Re, and (Rn-G)), finding that the EF(Rs) method yielded relatively better accuracy in daily ET simulations.”. Although this evaluation was at global scale, it only used three EF methods.

Therefore, this study is focus on evaluating the performance of seven upscaling methods at global scale. This is where our study is different from previous studies. The results are shown in the section 3.1 Intra-day distribution of observed LE and its influencing variables, 3.2 Accuracy of seven upscaling methods, and 3.3 Spatial distribution of the accuracy. This study also investigates the performance under non-clear sky conditions and 'time of satellite overpass', which are shown in the section of 3.4 and 3.5 respectively. In addition, Variability of simulation accuracy among different upscaling schemes and sites is also shown in the section 3.6.

According to your valuable comments, the author would like to modify the fifth paragraph of the introduction, so that the characteristics of this study can be more clearly described.

2) Did you check the energy balance closure of the FLUXNET data? And if it did not close, did you use the raw data or some kind of corrected data (e.g., assigning the gap in the SEB via the bowen ratio to H or LE?). Please explain and discuss how and if this affect your results.

Reply: Both of the raw latent heat flux data and the corrected data are included in the FLUXNET data. Corrected data by energy balance closure correction factor was used in this study. This is described in P4 L120.

This study tried to compare the evaluation results between the two data, and found that there was little difference between the two results, when the criteria used the root-mean-square error (RMSE), Nash–Sutcliffe efficiency (NSE), and determination coefficient (R^2). The difference of the evaluation results was relative greater when the criterion is the relative error. The difference magnitude was consistent with the energy balance closure correction factor. Overall, the difference could be found from the equation (8) (P6 L157). In this equation, the ratio of V_d to V_i is the same for simulations from two data. Therefore, the difference between two data simulations is expected to be the same as that between two data series themselves. The author would like to discuss these in the manuscript.

Other comments:

- P2L51: I am not that happy with the acronym LE in case L is not equal to λ. In my opinion it is most clear if you define the latent heat flux as ρλE in W/m2, where λ= the latent heat of vaporization in kg/J, ρ density of water in kg/m3, and E the evaporation in m/s.

Reply: The acronym LE is referenced from the FLUXNET data document. They defined the latent heat flux as LE. This acronym is common in many literatures. The author would like to modify this sentence "the EF was defined as the ratio of the latent heat flux (LE=λET, where λ is the latent heat of vaporization) to the available energy flux (Rn-G) at the surface." to "the EF was defined as the ratio of the latent heat flux (LE=ρλE, where ρ and λ are the density of water and the latent heat of vaporization, respectively) to the available energy flux (Rn-G) at the surface." Do you think this modification is appropriate? Or could you please give some suggestions about this modification?

- Eq2+3: What is the unit of LE_t? If this is a scaling factor (and thus dimensionless), I

would recommend to change its name. Earlier you defined LE as the latent heat flux (in W/m2), so better to redefine it. Furthermore, 't' is not defined.

Reply: Yes. The name in these two equations needs to be changed. According to your valuable comment, it might better change the LE_t in Eq2 and Eq3 to $SINE_t$ and $GAUSSIAN_t$ respectively. $SINE_t$ and $GAUSSIAN_t$ represent instantaneous sine and Gaussian function values during daytime, respectively. "t" represents time. As you mentioned that, they are scaling factors, and dimensionless. The author would like to make revisions. The corresponding Eq4 would also be revised by the new names. In addition, the sum in Eq4 would be replaced by the average.

*-Eq7: I would add ρ (density of water) to the LHS of eq7. Also I don't like the term ET for evaporation. It can be confused with E*T. Better call it E.*

Reply: The author would like to modify the "λET" to "λρE" in Eq(7).

- P10 L255-262: Other cause can be that remote sensing products only sense the top of the canopy and thus ignore the energy storage with in the canopy. Especially for forest this can be significant. See e.g. Coenders-Gerrits et al./ Jiménez-Rodríguez et al plus references herein.

References:

Miriam Coenders-Gerrits, Bart Schilperoort, César Jiménez-Rodríguez "Evaporative processes on vegetation: an inside look" (2020). pp 35-48. Book chapter in "Precipitation Partitioning by Vegetation: A Global Synthesis", editors John T. Van Stan, II; Ethan D. Gutmann; Jan Friesen; Springer.

Jiménez-Rodríguez, C. D., Coenders-Gerrits, M., Wenninger, J., Gonzalez-Angarita, A., and Savenije, H.: Contribution of understory evaporation in a tropical wet forest during the dry season, Hydrol. Earth Syst. Sci., 24, 2179–2206, https://doi.org/10.5194/hess-24-2179-2020, 2020

Reply: The author studied these two references carefully and found that it's true. Remote sensing products only sense the top of the canopy and thus ignore the energy storage with in the canopy. Especially for forest this can be significant. According to your valuable comments, the author would like to added this reason and references into the manuscript.

- P11 L293: Personally, I found an error of 36,7-25% not really 'satisfactory'.

Reply: This sentence is based on the previous sentence "When $0.4<\tau<0.5$, the simulated NSE had improved to exceed 0.70, and the corresponding R2 was greater than 0.75." The relative error is approximately 10% (Figure 7). The words "low atmospheric transmissivity." in this sentence are tend to confuse readers. The author would like to modify this sentence to "This indicates that remote sensing ET upscaling methods can achieve satisfactory simulation accuracy even when $0.4<\tau<0.5$."

- Fig 3: Should the difference between S and M not be mentioned in the method section?

Addtionally, it is logical that the error of M is lower than S if you follow the theory of error propagation. In M you have n=3 and thus the error reduces with a factor 1/√n.

Reply: As mentioned in the caption of Figure 3, "S and M represent simulations by a single and multi-time values, respectively. For example, S10:30 is simulated by the ratio of a daytime value to a single time value at 10:30, while M10:30 is simulated by the ratio of a daytime value to the average of three-time values at 10:00, 10:30, and 11:00." M means that the daily LE is simulated from the average of three-time values, but not from the sum of three values. According to your valuable comments, the author would like to modify the sentence in the caption "S and M represent simulations by a single and multi-time values, respectively." to "S and M represent simulations by a single and average of multi-time values, respectively." to avoid misunderstandings. In addition, as mentioned above, the sum in Eq4 would be replaced by the average.

- Fig 6: you interpolated the RE and RMSE over the entire world. But is this not visually biassing your graph, since the global coverage of the FLUXNET data is not equally distributed over the world?

Reply: Yes. This may not be appropriate. The author would like to modify Fig 6 similar with Fig 4. The figure would show only the evaluation results of the observed sites, but not the interpolation results.