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Discussions

Interactive comment on "Effect of the Revisit Interval on the Accuracy of Remote Sensing-based Estimates of Evapotranspiration at Field Scales" *by* Joseph G. Alfieri et al.

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Subject of this study is the error introduced by 'temporal upscaling' of daytime evaporation as a function of the length of the interpolation interval. Temporal upscaling is used to estimate evaporation on days at which no direct evaporation data is available as E = f*X. X, the reference quantity, is known on a daily basis, the scaling factor f is assumed to vary only gradually over time and is determined through interpolation. The analysis is based on solely in situ measurements of meteorological data, turbulent fluxes and the ground heat flux, from 20 Ameriflux sites. Interpolation intervals of 1 to 32 days and five different reference quantities and associated scaling factors are considered, using three interpolation techniques and four metrics to evaluate the self-persistence of the

scaling factor and error in the scaling factor and evaporation estimate. The results are analyzed per land cover class (mean of 4-6 sites per class). The authors found that the autocorrelation of the scaling factor is limited for lag times larger than a few days; that the error in both f and E quickly increases with increasing interpolation interval during the first week, more slowly afterwards; and that both the lag time for a certain autocorrelation threshold and the interpolation interval for a certain relative error in the evaporation estimate is smallest for forests, followed by cropland, grassland and open canopy. The authors relate the differences between land cover types to the mean latent heat flux per land cover type (which is largest for forest, smallest for open canopy) and define an exponential function relating mean evaporation per land cover type with a maximum interpolation interval to achieve a relative error of x%. Using the exponential relation they define a maximum return interval of 5 days to achieve a relative error in the evaporation estimate of maximally 20%.

The question the authors address in this paper is relevant for various applications of daily evaporation time series in e.g. water resources management and hydrology. The use of the relatively large set of Ameriflux sites covering different land cover types, climates, soil types, and covering many measurement years, makes the results – in potential – also more generally applicable than other literature on the subject. The authors however focus mainly on the practical implications of the results (selection of best interpolation technique and maximum return interval for a given accuracy of the evaporation estimate), than on understanding the why of the results (e.g. what actually explains the differences they find between and within land cover types and scaling approaches). This I think is a missed chance, given the data they have and the analysis they have already performed. The approach of Farah et al. (2004) for example, showing the seasonal variability of the evaporative fraction and trying to explain it, I find very useful in this respect as an additional analysis. Moreover although 20 Ameriflux sites 'distributed throughout the contiguous United States' have been used, the only information on the differences between the sites that is provided by the authors is the land cover class. The reader therefore cannot fully judge the representativeness of the results of this study, whereas given the practical focus of the analysis that should be a major benefit of this study. Of the comments below, at least comments 1,2,(3),7 and 8 I think should be addressed before publication of the manuscript.

Specific comments

1. The title of the article, mentioning the accuracy of remote sensing-based estimates of evapotranspiration, is a bit misleading. Of course the results of this study provide an indication of the effect of the revisit time on the accuracy of RS based estimates, but this study does a) not actually use remote sensing based estimates, and b) the point scale results are not one-to-one transferable to remote sensing based results. I suggest to change the title, leaving out remote sensing.

2. In the Introduction, p5, it is stated that perfect retrieval of the flux is assumed. If indeed the results found in this study are to be transferred to remotely sensed-based evaporation estimates, could the authors elaborate a bit on how realistic this assumption is, either in the introduction or in the discussion of the results? In other words, to what extend can the results based on the in situ, point scale measurements be applied for remotely sensed, usually more spatially aggregated, estimates?

Methods

3. I understand daytime mean evaporation instead of daily mean evaporation is used because of the better representativeness of the instantaneous scaling factor during satellite overpass. For many applications however (hydrological), daily evaporation estimates are valuable. It would be interesting if the authors could show whether the results for daily evaporation are very different. (Although also EC measurements are not that perfect in measuring nighttime fluxes (Fisher et al. 2007).)

4. I like that different ‘reference quantities’ are analyzed, but are these selected because these are the ones that are used most often (so for application purposes mainly), or could the authors also elaborate a bit more on the expected differences in self-

preservation of the associated scaling factors?

5. I see value in using different evaluation metrics as long as the added value is clear. I wonder whether the authors could better explain the added value of D next to the RMSE and especially MAE, either in the methods section or in the results section. As it is presented now, D is just another metric, confirming the results of MAE.

Results

6. All results are shown as the mean value per land cover class. Although it is a logical choice to group the measurement sites per land cover class, I expect there to be considerable differences between the sites within a class reflecting differences in e.g. climate and other environmental conditions (as visible in Fig.1 as well). However, the authors do not comment on the ‘within class’ variability of the autocorrelation and for the remaining results the variability is not shown at all. I suggest to at least provide some additional information on the Ameriflux sites in Table 1 (at least climate and seasonality information) and to include information on the within class variability of all results, to inform the reader on a) the robustness and b) the representativeness of the analysis.

7. In Figure 2 and Figure 9 the maximum lag time for a certain threshold of the autocorrelation and maximum return interval for a certain threshold of the relative error in evaporation estimate are plotted against the mean latent heat flux per land cover class. First, continuing on point 7, I wonder why the authors choose to plot the results for the mean latent heat flux per land cover class and not for all individual sites? Second, does there exists a similar relation between maximum lag time or return interval and latent heat flux for the data of one site? Furthermore, I would like to know whether the authors did consider other dependencies of maximum lag time (Fig.2) and maximum return interval (Fig.9) than the mean latent heat flux?

8. Page 8-9. The authors show and describe the results concerning the RMSE and MAE of the scaling factor as a function of itself (75% of its maximum reached after

xx days). Could these values be related to the absolute value of the scaling factor as well? Or in other words, how does the relative error in f develop as a function of the lag? Idem for the evaporation estimates (Page 9).

9. I think that it may not be necessary to show Figures 5 and 6. The results for the scaling factor already provide enough ground to prefer linear interpolation above spline interpolation methods, so Figure 7 summarizes the relevant information on the development of the error in evaporation estimates with increasing interpolation interval. (Although it is interesting why the MAE in the evaporation estimate for forested sites is lowest for Rn as reference quantity (compared to the other reference quantities), whereas the MAE for the associated scaling factor is largest of all scaling factors?)

10. As I understand, in the analysis as presented here, all measurement days are grouped without differentiating between e.g. seasons, phenological stages, sowing and harvest dates, cloudiness (apart from on days that are used for interpolation) and moisture availability. I'm curious whether the authors did consider doing the analysis for e.g. the growing season only, since the variability of the evaporative fraction most likely changes with the seasons (water availability, phenology (e.g. Farah et al. 2004). For applications in water resources management one might be especially interested in evaporation during the growing season and I wonder whether the maximum interval would be different.

11. It would be informative if the authors could include some figures showing the actual latent heat flux for all land cover types or preferably sites (e.g. the average annual cycle per land cover type/site), so that the reader gets some more insight into the daily and seasonal variability of the latent heat flux and evaporative fraction itself, before showing the thereof derived analysis of self-preservation of the scaling factor and interpolation errors.

Minor and technical comments

12. Although one can figure out what is meant, the authors may want to go through the

introduction again and clarify / restructure the text here and there. For example page 6, lines 19-26 could be rewritten a bit more clearly. I understand it like this: Based on the daily time series time series with intervals of 2 to 32 days are generated. Per interval of length n we generate the n possible unique time series starting at the first n days of the daily series, to maximize the robustness of the statistical analysis. The generated time series were subsequently filtered for clouded days.

13: I would include the variable and unit on the y-axis, at least in the top left panel of the figure (in stead of fx, everywhere).

14. Page 4, line 15: I think Lhomme and Elguero (1999) were one of the first to describe the influence of clouds on the self-preservation of the evaporative fraction? I suggest to include the reference and maybe skip the word recent.

15. For clarity, could the formulae of the five scaling factors and reference quantities be provided? Page 5, line 26: how is the available energy defined? I suppose you use A = Rn-G? Or are other (e.g. change of energy storage in the canopy) terms involved?

16: Page 6, line 28. The threshold for a cloud free day seems to tolerate quite some cloudiness? On what is the 25% based?

17. Page 7, line 1: What is the total length of the observation time series after removal of the clouded days?

18: Page 7, line 22-26. The authors could better explain the use of the ‘necessary transforms’.

20: Typos:

Page 3, Line 31: presence *of*, Line 33: referred to *as*.

Page 4, Line 22: dot behind (Van Niel et al. 2012),

Line 26: For example, [*something missing?*] and Tasumi et al, Line 30: no dot behind time

Page 5, Line 13: Delete *Namely*,

Line 18: while/by maintaining

Page 7, Line 22: Once calculated for *the* individual the sites

Page 8. Line 13: Derived *from* meteorological data

Line 14: associated *with*

Line 28. ‘Interpolated estimates of each *X*’, must be ‘each *f*’ I think?

References

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