Hydrol. Earth Syst. Sci. Discuss., 10, C6050–C6056, 2013
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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Upscaling of evapotranspiration fluxes from instantaneous to daytime scales for thermal remote sensing applications" *by* C. Cammalleri et al.

Anonymous Referee #2

Received and published: 1 November 2013

Overview and general comments:

Thermal remote sensing data have great potential for modelling spatially and temporally dynamic actual evapotranspiration (ETa). How to best 'upscale' an instantaneous observation or remote sensing estimate to a daily or longer interval is still an open research question, even after many years of study. This paper evaluates 4 upscaling methods that estimate daily ETa from single time-of-day latent heat flux observations. The four methods are based on an assumption of self-preservation with some other observed or modelled surface flux that should supposedly be easier to observe or model than the latent heat flux itself. The single time-of-day observations of latent heat flux

for this study were from 12 eddy covariance flux sites from the Ameriflux dataset. The flux measurements were from daytime only, and the intent was that these observations should reasonably represent the evaporation that could be modelled by thermal remote sensing data. A major part of the experimental design was around assessing the impact of (uncertainty around) forcing the energy balance of the flux tower data to close (or not). It was concluded that at-surface solar radiation was the most robust scalar of ETa.

Overall, I found the results presented to be interesting and worthy of note. In general, the paper was well written, but it was not particularly well structured. There were several instances where presentation of methods, results, and discussion were found in inappropriate sections, which lead to unnecessary confusion. I feel that certain sections of the paper need re-writing. I provide my more major concerns/comments in this report and have marked a copy of the paper with my minor comments.

Major comments:

I have 9 major comments, which I summarise below:

(1) My biggest technical concern about the paper is that I wonder whether the results are confounded by temporal influences due to geography. The authors should normalise the 7 times of daytime by local sunrise and daytime length so that future readers need not have the same question about the analysis. If done, it would mean that the paper would have higher potential impact as the results would be presented in a way that was more generically applicable and less site specific.

The results of the paper are presented based on 7 local times throughout the daytime period. In each case, overall summaries of all 12 flux tower sites were provided. These 12 sites may have some potentially large longitudinal differences (although I am unsure of what sites were used as Table 1 was missing and there was no figure of flux tower site locations). The 7 daytime times were not normalised based on a given flux tower site's time after sunrise nor on daytime length. Because the times are not normalised, it makes me wonder if the results and conclusions drawn from these overall summaries are substantially influenced by the seasonal and longitudinal/latitudinal differences at the 12 difference flux sites.

As an example, let's consider two hypothetical sites within the same time zone (UTC–5, eastern time zone of US), one in Maine, USA and one in Michigan, USA. These two sites have roughly the same latitude (e.g., $\sim$46°), but Maine is near the eastern extent of the time zone (e.g., Longitude $\sim$ –68°) , and Michigan is near the western extent of the time zone (e.g., Longitude $\sim$ –86°). These two sites will have the same daytime length (in December $\sim$8.75 h, in June $\sim$15.75 h), but apparent sunrise will be about 1.25 h different from each other. Daylight savings time will need to be accounted for in June, but not in December at both sites. The normalisation formula is t* = (t – t_sr)/N where t* is the normalised time, t is the local time, t_sr is the local time of sunrise, and N is the daytime length (all in units of hours). Let's consider 12 noon at both sites. The normalised time, t* for Dec. and Jun. at both sites would look something similar to the following:

SEE ATTACHED FIGURE

Noon is nominally regarded as the half-way point of the daytime (i.e., t*=0.5), whereas the table shows it is ranging between 0.38 and 0.57 for our simple example. Probably, this influence is marginal on the analysis performed, but perhaps not. A potentially worse influence might be found relative to latitude if low latitude sites and very high latitude sites are compared. Seasonal influence on latitude will affect the daytime length. My concern is that these influences might add a good deal of noise into the anlaysis and could potentially be problematic for the interpretation of Fig. 3, Fig. 4, and Fig. 6 where specific time of day and monthly influences are being assessed. As I said before, it is probably OK, but when attempting to make scientific comment, it is best to remove these potentially confounding factors, especially when they are known and when they are easy to account for.

(2) Eq. (1) considers a correction factor, β. For three of the upscaling approaches, β was set to 1. For the EF approach it was set to 1.1. It was stated on p. 7333 that setting β=1.1 improved the accuracy of the EF approach. It seems like a missed opportunity to not summarise what the optimal β was for each upscaling approach and to report how much it improved their performance. I would really prefer to see some formal analysis done on β. Otherwise, the very many references to this parameter are not really well tied in with the actual analysis of the paper.

(3) Following on from point #2. β is the correction factor for upscaling using a method based on self preservation. This means it is intrinsically related to the errors calculated in this paper. In order to further link the paper in to this correction factor, the two error equations could be easily written in terms of β. I suspect that this would add context to the paper that seems to be missing at the moment. The two equations are currently written like:

MAE = E|ETd_x – ETd|

MBE = E(ETd_x – ETd)

Whereas it would link in to Eq. (1) better if written like this:

MAE = E|ETd_x – ETd| = E|ETd *(βˆ(–1) – 1)|

MBE = E(ETd_x – ETd) = E(ETd *(βˆ(–1) – 1))

(4) The paper needs to be gone over with careful attention to detail. There are several places where the standard publication 'protocol' is breached. For example, (i) the second paragraph of section 3.1 shows and discusses results within the methods section (I can understand why the authors have done this, but it should be easy for them to present these results in the results section rather than the methods section), (ii) the last paragraph of section 4 introduces methods within the results section; (iii) p. 7336 near line 25 introduces methods in the discussion section; (iv) p. 7337 near line 10 introduces methods and presents results in the discussion section. There may be

other examples – the authors should find any such instances and move them to their appropriate place within the paper. Having methods described in sections other than the methods section causes unnecessary confusion. The same goes for presenting results in sections other than the results section. Adhering strictly to standard protocol will greatly enhance the communication of this paper.

(5) Section 3.2 is arguably the most critical sub-section to understand of this paper. I found this sub-section to be the least clearly presented. I suggest taking time to make this section better. The approach taken is simple, yet I think very useful, but it needs to be clearly presented. Also, it would be preferable if the choice of the value of delta was justified somehow. It seems rather ad-hoc. I would also much prefer if the equations were not imbedded in the text, but rather if they had equation number. I suspect it would help communication of this important part of the paper.

(6) Sub-section 3.1 seemed to be lacking in detail. The second paragraph (of two), as discussed above, was not even presenting methods and so doesn't belong in this sub-section. Again, moving the equations from imbedded within the text to having their own equation numbers might help some.

(7) In the discussion section, the limitations of the EF, R_TOA, and REF approaches are discussed, but the limitations of the RS method are not, as far as I can see. The RS approach is based on observation. How would someone make use of this approach operationally (e.g., upscaling continental remote sensing based ET)if either the observations was missing or if the area of interest is far away from an observation?

(8) In the very last paragraph of the discussion section, good points about daytime versus 24-h upscaling of ET are made. However, I feel it is important to make the point that for practical hydrology, 24-h ET is really what is needed, not daytime ET. For example, if attempting to perform large-area water accounting, daytime ET would not be directly useable for modelling the water balance (nighttime ET would then also need to be modelled). So, while the point may be true that inclusion of nighttime EC fluxes

can cause greater uncertainty in the analysis, the counterpoint that nighttime ET must be accounted for and studied should also be made here.

(9) I really feel that the conclusions should be re-written. It would be much more effective communication if the conclusions were restricted to things that were actually studied in the current paper. Right now the conclusions are, in my opinion, 50% conclusions about the analysis and 50% a second discussion section. The conclusion section is not really meant to be another discussion section, but rather a synthesis of the important findings.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C6050/2013/hessd-10-C6050-2013-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7325, 2013.

Site	Month	t_sr	N	t	t*
Maine	Dec.	7	8.75	12	0.57
Michigan	Dec.	8.25	8.75	12	0.43
Maine	Jun.	4.75	15.75	12	0.46
Michigan	Jun.	7	15.75	12	0.32

Fig. 1.