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Discussions

Interactive comment on "Robust historical evapotranspiration trends across climate regimes" *by* Sanaa Hobeichi et al.

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General comments

- The authors provide a new ET data set, making use of many parent ET data sets and in-situ ET measurements, at the same time avoiding information redundancy and exploiting information which is either missing in other parent data sets or has a large bias in comparison to the in-situ observations. The methodology provides a wealth of ways to improve DOLCE v2 as compared to DOLCE v1 and other ET data sets. They show both an impressive literature research and validation of this new ET data set. After the validation they compute historical trends, which show that across most of the regions globally, ET is to increase.

- Because of the efforts made to improve on DOLCE v1, a big part of the paper is about verifying DOLCE v2 against the parent datasets and in-situ observations. I think the title should reflect that.
- The authors put a lot of effort on trying to find ways to improve DOLCE v2 in comparison to DOLCE v1 by i) weighting groups (Figure 1; right column) and ii) Bias correction strategies (Figure 2; Supplementary Figure 3) as described in 2.2.4, 2.2.5 and 3.1. Despite the authors efforts to significantly improve the ET estimates, I think that the added complexity does not justify the little improvement gained. I would suggest moving sections 2.2.4, 2.2.5 and 3.1 and corresponding figures to the Supplementary materials. In the main text, the authors can shortly motivate the weighting group and bias correction strategy by referring to the Supplementary materials. This would make the derivation of DOLVE v2 ET more straight-forward and benefit the readability of the manuscript.

Specific comments

- Throughout the paper, please properly introduce i) table contents, ii) figure axes labels and color codes and iii) statistics of box-plots.
- lines 20: I found the notion that these climatology clusters / climate regimes are able to summarize or even replace the Köppen-Geiger climate regimes quite interesting. That would be worth a mention in the abstract, also putting this sentence into context.
- lines 23-25: "We find that despite robust . . . ET clusters". This is the only time this is mentioned in the entire manuscript. I don't see the relevance of it for the abstract.
- lines 82-84: FLUXCOM also belongs in this summation of gridded datasets that successfully exploit in-situ measurements.

- lines 198-200: I assume that the 'very large spatiotemporal domains' are equal to the spatial and temporal resolutions of the ET data sets? Do the authors mean that through time and varying wind directions, you might actually get closer to the grid cell mean than looking at individual days?

- lines 223-230: Just out of interest: What is the total amount of days initially available from all sites? After the filtering based on data availability, how many days are left?

- line 241: Could you elaborate on these conditions? As I'm not a flux tower measurements expert: Are these 20 and 30 W m-2 thresholds usually applied? Is there a paper where this methodology is also applied?

- lines 246-249: How do you justify using LE values without any correction – any value is better than nothing? Did you verify the differences between i) only LE data with correction and ii) LE data with and without correction? Are there any biases there?

- lines 250-255: Why not just take an average of the different towers within one grid cell, weighted by fractional cover of biome within that grid cell? I would assume that you want to have the possibility of retaining as much data as possible, as there could be data gaps in the data records in the tower measurements which could be filled with values from towers in the vicinity. And an additional question: Do you somehow account for varying footprints for the in-situ ET measurements? Depending on the location of the flux tower within the grid cell, what the tower measures could be from a neighboring grid cell.

- lines 272-275: Are w_k are the weights, which sum to 1, based on the error correlation in Supplementary Figure 2? If so, please state that here for clarity.

- lines 296-301: "the discrepancy between DOLCE and actual ET at any spatial scale greater than that of a tower footprint should be less than this uncertainty

estimate" I am slightly confused here: This would only be true for spatial scales greater than the tower footprint and smaller than 0.25x0.25 degree grid cell resolution (which is used in the study), right? If the spatial scale would be greater than 0.25x0.25 degrees, the discrepancy should be even larger?

- lines 351-353: This might be a naïve question, but is that not just because of the small number of flux towers on the SH? I could imagine that due to the higher availability of in-situ measurements and abundance of other measurements networks ET estimates are much more well constrained in the NH than the SH.

- lines 357-359: I was confused here, as I assumed the authors were referring to boreal summer. Please clarify.

- lines 380-282: I do not understand what is meant with 'extrapolating the bias field'. Please explain more clearly.

- Figure 3: Does zonal ET follow a Gaussian distribution? If not, it would make more sense to define the grey ribbon as the interquartile range instead of the standard deviation. Also, I would suggest making a mask of the grid cells where all of the parent data sets have values, so that a comparison is fairer. The figure without the mask could then be moved to the supplementary material.

- lines 500-501: Mentioning the seasonal cycle of DOLCE v2 ET but not elaborating on it feels out of place. Either elaborate on differences between seasonal cycles between DOLCE v2 ET and others or remove.

- lines 525-528: Please clarify this sentence; I found it confusing as written.

- lines 540-542: Either put into context by comparing with all the other literature references or remove. These two sentences seem lost.

- lines 546-550: In the first sentence the authors state the RMSE is not computed because the means between DOLCE and sites are not equal. In the sentence after you explain that all data has been normalized and therefore all have a zero mean. So, by normalizing you could in principle calculate the RMSE?
- lines 558-560: Would the signal of land cover be clearer when the authors would aggregate the land cover types to short/tall vegetation? Next to that, in Supplementary Figure 6, the color legend is blocking some of the extreme values.
- lines 571-572: Is there a specific conclusion drawn from these figures? Otherwise mentioning them seems unnecessary to me.
- Have the authors also looked at ET trends from flux tower measurements? As trends across all KS-clustering defined climate regimes are positive, the flux tower observations could corroborate that if they are also generally positive, right?
- line 678-680: I don't know how the fact that the global ET trends are different than the other ET products reflects usefulness; the fact that the DOLCE trends are different does not necessarily mean they are correct. However, it would be really interesting to see whether flux tower observations find similar long-term ET trends.

Technical corrections

- line 21: 'at each location'. Do you mean globally?
- line 42: remove the comma after 'approaches'
- line 113: replace 'trends (5) behavioural' with 'trends and (5) behavioural'

- lines 236: to avoid confusion, maybe rephrase "latent heat measurements are used directly" to "latent heat measurements are used without any corrections".
- line 435: Replace 'Fig. S3' with 'Fig S4.'
- line 590: replace 'intensified' with' increased'
- line 624: replace 'modifed' with 'modified'
- line 743: replace 'Figure2' with 'Figure 2'

I do not wish to remain anonymous - Jasper Denissen

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