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Discussions

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Interactive comment on "Reviews and syntheses: Turning the challenges of partitioning ecosystem evaporation and transpiration into opportunities" *by* Paul C. Stoy et al.

Anonymous Referee #1

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General comments:

The manuscript reviews Evapotranspiration partitioning methods, with focus on the most recent ones; and because of the links to photosynthesis and physiology it is well placed in BG. Beyond a pure review, interesting and novel own considerations of the authors are added which might help to test and improve these methods in the future.

It is well written, and gives an impressing complete and detailed overview on those aspects of ET partitioning the authors chose to treat in-depth. Concerning this choice, I have one general comment on the labelling/scope/structure of the manuscript. The title and large parts of abstract, introduction and background seem to suggest that the

whole field of ET partitioning is subject to the paper. Later during the manuscript it becomes clear that three categories of partitioning approaches are treated differently profound.

First, in section 3.0 a number of methods are mentioned to be outside the scope of the manuscript (because of multiple recent other reviews), but nevertheless most of them are more or less briefly mentioned in the following paragraph, which is a bit confusing. Maybe it would help clarity to either not write anything about them that goes beyond a mere list, or to treat them a bit more detailed (e.g. with one reference per method) but then adapt the way they are placed in context of the paper (i.e. exchange "not our intent to reiterate them" for something like "will only give a brief overview", or put this statement after the list of methods rather than before it). Also, the role of bulk ET methods such as watershed residual or scintillometry, while surely worth mentioning somewhere in the MS, is not clear at its particular place between the above statement and the partitioning methods. If it stays here, it should be better linked to the text around. Finally, I wonder whether subcanopy EC measurements (now in 3.1 at p6L6) might better fit in this section too. Technically, they are indeed half-hourly EC observations once they have been installed, but this is the case at few stations, done deliberately for partitioning, they have a small footprint, and no further connection to what is discussed in 3.1 (or the rest of the MS before Sect. 5). In all this, they resemble the methods in 3.0. I am not sure whether "scale" is the ideal criterion to distinguish 3.0 from the other methods, but I have no better suggestion either.

Second, there are methods in section 3.2 to 3.7 which are treated as in a typical review – summarizing the latest state of the art very well as far as I can judge; maybe a bit more detail and explanation would help at some points.

Third, the methods in section 3.1 appear to be at the heart of the MS. They are not only summarized very thoroughly, but section 4.1 and 4.2 also present considerations that, while of general interest, are particularly valuable for the assessment and improvement of these methods (and to some degree of those in 3.2).

I would like to encourage the authors, if not amending the MS such that all methods receive similar attention (which would be a major revision and surely is an option, but probably not the intention and maybe not so interesting given the existing reviews), to think where minor revisions to the wording of title, abstract, intro and background can help give readers a clearer impression of the focus of the paper.

Specific comments:

p6L20: reword or explain in more depth the deltas and the word marginal in this specific context

p6L25: Maybe VPD instead of D would help readers easily recognize the variable all over all over the manuscript?

p8L4: The title "Advanced algorithms for partitioning eddy covariance data" is somewhat arbitrary as a distinction from the section before. Maybe something like "Partitioning ET using high-frequency Eddy-covariance raw data"?

p8L25: The better results during fair weather were not a result of the LES comparison. One more maybe noteworthy result of the LES comparison was, however (if it is not too detailed for the intention of this review) that an assumption about transfer efficiencies in the original Scanlon approach is frequently violated.

p9L16: The various definitions of ecosystem- (as opposed to leaf-) level WUE seem to become more and more confusing. After WUE_{eco} = NEE/ET (Scanlon and Sahu 2008) and WUE_{eco} = GPP/ET (Beer et al. 2009, Global Biogeochem. Cycles 23: GB2018), this one is already the 3rd. While this is not the fault of the review authors, they might want to take the opportunity to try to order them a bit or at least mention the variety of existing definitions. The three above, in that order, can be thought of as increasingly close approximations of leaf-level WUE. While all have their methodological justifications, it seems counter-intuitive to label the one closest to leaf-WUE (i.e. the 3rd) as "ecosystem-level". IMHO a reader coming across that term for the first time,

would rather expect it to indicate the ratio of CO_2 gained (in net) by the ecosystem to vapour spent by the (whole) ecosystem, i.e. the first one (NEE/ET).

p11L1-13: The first paragraph summarizes how satellite-based remote sensing can be used to quantify bulk ET (without mentioning partitioning) while the second one on partitioning seems to apply only to much lower/closer remote sensing platforms (tower or airborne). If this is true, please try to put more clearly. Otherwise (e.g. if the separate E and T estimates occurring in some satellite-based ET algorithms have been proposed as serious partitioning method, rather than just means of minimizing the bulk ET error), add such information.

p13L12: Consider comment p9L16 as to how to call this type of WUE.

p13L16-19: The methodological details of this interesting approach and their effects on interpretation could be elaborated a bit more. Was T for the left-hand side of the equation / the Y-axis of the figure explicitly needed? My guess from the text (but this is not completely clear) is that you used GPP/ET from the Fluxnet dataset, and the task (or at least one of the tasks) of the 95-percentile separation was to extract the data points where T->ET. If, in contrast, T was explicitly determined from the EC data, which method was used to avoid any circular reasoning? Also, it would be interesting to learn whether the 95-percentile rather ruled out specific ecosystems (that fail to behave "optimally"), specific meteorological situations, or a mixture of both. Is the result sensitive towards changing the percentile (e.g. 90 or 99 %)?

Fig2: Are the restrictions mentioned in the caption (solar zenith angle, soil heat flux, ecosystem info) motivated by this study? Otherwise it may be sufficient to mention the dataset from Stoy et al. 2013 was used, or to mention somewhere (caption or discussion text) the only one(s) that might possibly have had an unwanted systematic side effect on the relation suggested by the figure (which is probably solar zenith angle).

Technical corrections:

p10L17: T in italics

p12L14: though => through?

p15L19: gages => gauges?

Fig. 2: Colour-coded values are probability densities with unit 1/([ET]*[D])? Is there one h too much in the unit of ET on the Y axis?