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Hydrology and Earth System Sciences Discussions

Interactive comment on "Analysis of the energy balance closure over a FLUXNET boreal forest in Finland" *by* J. M. Sánchez et al.

Anonymous Referee #3

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General Comment

This manuscript examines the energy balance over a boreal forest and provides a reasonable discussion of many of the issues associated with the now familiar and well known problem of lack of closure. Although I find the basic discussion and analysis of the paper reasonable, I also think there are some critical issues that need to be included. I recommend publication after some important revisions. My specific comments are listed below.

Specific Comments

(1) Page 2685, last paragraph (soil heat flux discussion) – Here the authors state that the heat flux plates are placed "at a certain depth in the soil to avoid disturbances, such

as the loss of contact…". But the loss of contact is always a possibility regardless of the placement depth of the heat flux plate. In fact, I rather suspect that partial contact is more the norm than the exception. So I would suggest that the authors revise the manuscript to allow for the possibility that the contact is incomplete. More specifically, they could assume that the G' is underestimated by 30% to 50% (which in my experience is not unreasonable) and see what impact this loss of contact may have on the energy closure. The authors also need to provide the mathematical details on how the soil heat storage was estimated.

(2) Page 2686, discussion of storage terms – I think the authors need to provide the details of their method for estimating EC storage-fluxes (SH and SLE). Are they just integrating the time rate of change of the canopy water vapor and temperature profiles? The lack of any biomass heat storage is unfortunate, so I think the authors need to include a statement that they will discuss the significance of heat storage in the biomass in more detail later in the paper (which, in fact, is what they do in the upper half of page 2695).

(3) Page 2686, discussion of ERB – I would recommend dropping ERB1 as a statistic. It is really not that useful of insightful. It might be more useful to include the 24-hour summation ratio as a measure of closure. The 24-hour summation ratio is $\sum$(LE+H)/$\sum$ (Rn-G-S), which has the nice property that $\sum$ (G+S) is approximately 0 for a 24-hour cycle, so that the 24-hour summation ratio is reasonably well and simply approximated by $\sum$ (LE+H)/$\sum$Rn. This approach obviates the need to include the storage terms and may help provide further insight into the nature of the lack of closure (that is, it is related to either the net radiation measurement or to the EC flux measurement, but is unlikely to be storage related).

(4) Pages 2687-2688, discussion of spectral corrections issues – (a) The authors should give some idea of the magnitude of the correction terms, especially if they are using a closed-path system to measure LE. Furthermore, readers should benefit y knowing some of the details concerning the authors' corrections for water vapor fluxes.

Massman and Ibrom (2008: Atmospheric Chemistry and Physics 8, 6245-6259) indicate that the closed-path LE flux corrections are likely to be dependent on humidity, as well as specific to a given EC system and to change over time. (b) Since the authors follow Moore (1986) they should be made aware of (if they are not already) that Moore's aliasing-related correction is an error and should not be made. Do the authors make include the aliasing correction or not?

(5) Page 2688, the paragraph beginning with the discussion of REBS Q-7 – My concern here is that all the major results reported by the manuscript are completely dependent on one measurement of Rn made by one type of net radiometer. The authors need to put this instrument and their closure results into the proper context. Specifically they need to answer how their closure results could have been different they used another type of radiometer or method for measuring Rn. There are some well known biases/discrepancies between different radiometers, as well as 4-way methods versus single net radiometers. How would the authors' ERB values and conclusions be affected or even changed had they used a different approach (or instrument) for measuring Rn?

(6) Page 2691, section 3.4 – The authors analyze their results relative to atmospheric stability (i.e., z/L). Here their basic assumption is that z/L is well measured. But if the EC measurements are underestimating H and/or LE, how well or precisely is z/L known? I do not think the closure-related problems are likely to affect the sign of z/L, but I am not sure how meaningful the upper and lower bounds they use for defining atmospheric neutrality (i.e., -0.01 and +0.01) can be. Again I think the authors should discuss this issue and provide some estimates of the uncertainties involved with their numerical estimates for atmospheric neutrality.

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