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Interactive comment on “New techniques for gap-filling and partitioning of H_2O and CO_2 eddy fluxes measured over forests in complex mountainous terrain” *by* Minseok Kang et al.

Anonymous Referee #2

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The paper proposes mainly a method to fill gaps and partition the water fluxes in evaporation and transpiration in particular in case of wet canopies. In addition it presents a modification in a data filtering method (ustar) that is used for CO2. New methods to partition water fluxes are for sure very interesting and important. For this reason the title is misleading since gapfilling and partitioning of CO2 are not discussed (or only marginally, without real new developments). Improvements are needed in the validation, explanation and uncertainty discussion. I suggest to reorganize it and focus mainly on the more interesting and innovative part (water), adding more sites in order to demonstrate that the approach is valid.

SPECIFIC COMMENTS:

Pages 2-3:The distinction between the ustar filtering and advection methods is not very clear and not fully correct. The ustar filtering has been proposed exactly to filter out data when there is advection so it in not true that it can not be applied when the “drainage flow is at night “. In addition what the authors call “Advection method” is in fact a partitioning method and not a filtering method (e.g. it assumes no advection daytime)

Par 2.3.2: there are a lot of parameters in the proposed model but it is not clear how they are estimated and also which is the associated uncertainty. Also it should be verified if the parameters are valid in different conditions (to test the “everywhere, all of the time” proposed by the authors P3L18). At the moment this is far to be demonstrated.

I find the section 2.3.3 not clear in the second part where the application of the model is reported. More information and a clear description of the procedure are needed.

Section 2.4.1 reports the NEE processing but the level of details is not sufficient to understand what is done (how are the parameter estimated? How is the ustar threshold calculated?). In addition three methods are presented as three independent approached but it is not explained if, when the light response curve method is applied, the data below the ustar threshold are removed (in this case they are not independent, if not removed that it is an error because it is known that the data are not correct). That said, I also find this part not relevant for the paper that is more focussed on H2O.

P7L28-30: authors should explain why the MPT method is “inappropriate for hilly terrain sites” while the ustar filtering (and so also the MPT) has been developed specifically to filter out the advection.

P7L31: the fact that near sunset the drainage has not yet completely developed must be proved.

P8L1-3: how can the authors be sure that the average nighttime flux in the two windows is not different for biological reasons? Or because of the uncertainty in the storage term? Or because a different wind direction distribution (i.e. footprint) is present in the two time windows?

P8L4-5: it is not clear what is proposed. If the two windows are different then all the data in the second window are removed? And ustar filtering is applied to what?

P8L18-21: is the model validation made using a leave-one-out method or using independent dataset?

Section 3.1.2: the comparison of the methods is no a validation and no strong conclusions can be derived from this. In addition, in the Figure 4 uncertainty is not considered to understand how much (if) the two approaches are significantly different. The only way to prove this is to add other sites, for example where close path IRGAs with heated tube are used (so that ET measurements are ok also with rain) and then apply the methods to artificial gaps.

Section 3.2.1: in Figure 5 the comparison of measurements before and after sunset is used to show the presence of drainage. However it is not clear if the data shown in the plot are filtered by ustar (they should otherwise the plot is biased by a known issue). The Table to add is the 2 and not the 1. Figure 6 doesn't show the original method and how much it is different respect to the modified. In addition it is not a validation because however compared with other models/methods. Figure 7: how was heterotrophic respiration measured?

Section 4: I find this section, although with some nice and interesting aspects, out of scope respect to the rest of the paper.

Appendix A, lines 22-23: the main reason of the minor effect of the storage method in ET is that the fluxes are always very low at night, when the storage component is important.

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